THE FIRST LACUS FORUM 1974

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edited by ADAM MAKKAI
and VALERIE BECKER MAKKAI

HORNBEAM PRESS, INCORPORATED
COLUMBIA, SOUTH CAROLINA

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FIRST EDITION

Published in Columbia, South Carolina by Hornbeam Press, Incorporated, 6520 Courtwood Drive, Columbia, S. C. 29206.

Manufactured in the United States of America.

CONTENTS

WHY LACUS? ix

I. The Rehumanization of Linguistics

1. *Victor H. Yngve*
   THE DILEMMA OF CONTEMPORARY LINGUISTICS *1*
2. *M. A. K. Halliday*
   LANGUAGE AS SOCIAL SEMIOTIC:
   TOWARDS A GENERAL SOCIOLINGUISTIC THEORY *17*
3. *Robert J. Di Pietro*
   THE NEED TO BE PRACTICAL *47*

II. Rumbles Within the Ruling Paradigm

4. *Dwight Bolinger*
   ON THE PASSIVE IN ENGLISH *57*
5. *Fred C. C. Peng*
   THE PLACE OF GENERATIVE
   PHONOLOGY IN THE HISTORY OF LINGUISTICS *81*
6. *Tej K. Bhatia*
   ON THE SCOPE OF NEGATION IN HINDI *120*
7. *T. D. Griffen*
   ON DESCRIBING THE CLUSTER PROSODY *140*
8. *Jean LeGrand*
   BOTH EITHER AND OR *148*
9. *Bernard Mohan*
   TESTING RULES IN FUZZY GRAMMAR *157*

III. Linguistics As Wissenschaftspolitik

10. *Raimo Anttila*
    REVELATION AS LINGUISTIC REVOLUTION *171*
11. *Uhlan V. Slagle*
    A VIABLE ALTERNATIVE
    TO CHOMSKYAN RATIONALISM *177*
12. *Will Baker*
    ON 'ADEQUACY' OF DESCRIPTION AND EXPLANATION
    IN THEORIES OF LANGUAGE ACQUISITION *194*

13. *Adam Makkai*
MADISON-AVENUE ADVERTISING: A SCENARIO *197*

IV. Philology and the Comparative Method

14. *Carleton T. Hodge*
A SET OF POSTULATES FOR COMPARATIVE LINGUISTICS *209*
15. *John Algeo*
THE ACRONYM AND ITS CONGENERS *217*
16. *Igor Miletic*
MARKEDNESS IN SERBOCROATIAN WORD ACCENT *235*
17. *Saul Levin*
GREEK OCCUPATIONAL TERMS WITH SEMITIC COUNTERPARTS *246*

V. Linguistics and its Interfaces

A. LITERATURE AND FOLKLORE

18. *Roger W. Wescott*
POETRY AS SELF-CREATION *264*
19. *Linda Dégh* and *Andrew Vázsonyi*
LEGEND AND MASS MEDIA *279*

B. PERCEPTION, KINESICS, AND TRANSMODAL CODES

20. *Laurence Urdang*
INFORMATION, NONINFORMATION, ZERO-INFORMATION, AND CONTENT *289*
21. *Walburga von Raffler Engel*
ON THE EARLY ACQUISITION OF KINESICS *298*
22. *James R. Martin* and *Sherry Rochester*
COHESION AND REFERENCE IN SCHIZOPHRENIC SPEECH *302*
23. *Douglas Vipond*
UNDERSTANDING AMBIGUOUS SENTENCES *312*
24. *Dale E. Woolley*
IMPLICATIONS OF THE STUDY OF AMBIGUITY AND HOMONYMITY FOR GRAMMATICAL THEORY *329*
25. *Peter A. Reich*
VISIBLE DISTINCTIVE FEATURES *348*
26. *J. H. Prost* and *S. L. Smith*
VERBAL RÉSPONSES TO BODY LANGUAGE *357*
27. *János Buttler*
ÉLÉMENTS DE L'ENCODAGE DANS LES LANGUES NATURELLES *372*

VI. Approaches to the Description of Natural Languages

28. *Valerie Becker Makkai*
'PRETTY DAMN SELDOM . . .': ON THE GRAMMATICALITY OF UNGRAMMATICAL UTTERANCES *386*

29. *John W. Oller, Jr.*
ON THE GENERATION AND MODIFICATION OF GRAMMARS *393*

30. *Ladislav Zgusta*
SOME OBSERVATIONS ON JESPERSEN'S ANALYTICAL SYNTAX *403*

31. *William Card* and *Virginia McDavid*
THE CLASSIFICATION OF RELATIVE CLAUSES *411*

32. *Harold Ekart*
A SYSTEMIC-FUNCTIONAL ANALYSIS OF GERMAN MODALITY *417*

33. *Charles Ruhl*
PRIMARY VERBS *436*

VII. Stratificational Solutions to Some Old and Some New Problems

34. *David G. Lockwood*
QUASI-ETYMOLOGICAL AND 'NATURAL' PHONOLOGY AS TWO VARIETIES OF THE SAME MISTAKE *446*

35. *Doris H. Welch*
LATINATE ENGLISH VERBS AND NOUNS: A SYNCHRONIC, DIACHRONIC, AND PANCHRONIC DESCRIPTION *458*

36. *William J. Sullivan*
ALTERNATION, TRANSFORMATION, REALIZATION, AND STRATIFICATION REVISITED *472*

37. *Rose Mary Babitch*
ON THE PEDAGOGICAL APPLICABILITY OF STRATIFICATIONAL GRAMMAR FOR CONTRASTIVE ENGLISH-FRENCH STUDIES *523*

38. *Earl M. Herrick*
ORTHOGRAPHIES AND SCRIPTS: A NECESSARY DISTINCTION *533*

39. *Sydney M. Lamb*
MUTATIONS AND RELATIONS *540*

WHY LACUS?

LACUS, the Linguistic Association of Canada and the United States, was inaugurated at Lake Forest College during a four-day session of papers and discussion, between August 18 and 21. This volume, The *First LACUS Forum*, contains the papers read at the conference as well as a few papers by LACUS members who, for various reasons, were not able personally to attend.

Our symbol, the Great Lakes, providing a natural border between Canada and the United States, stands for more than the convenience of acronymy (about which see John Algeo's paper in this volume). It is a major international frontier, one which not only unites these two giant nations of North America, but also North America with Europe and the rest of the world. It is possible to board a boat in Chicago, sail up to Montréal, then enter the Atlantic, and from there sail to any port in the world. Additional associations with men of letters and inland waters suggest themselves - - - (e.g., the 'Lake Poets' of England), and one of the avowed intentions of our society is to broaden the scope of linguistics, bringing it out from its narrow parochialism. The papers by Roger W. Wescott 'Poetry as Self-Creation' and by Linda Dégh and Andrew Vázsonyi 'Legend and Mass Media' as well as the paper by Jack Prost and Sandra Lee Smith 'Verbal Responses to Body Language' indicate that the *First LACUS Forum* has seriously tried to expand the gates of knowledge and practice interdisciplinary linguistics to a presently unprecedented degree.

One of the most significant aspects of this meeting was that the participants who came to discuss their views were truly able to exhibit a spirit of cooperation, friendship, and constructive criticism, while allowing every participant to practice his or her particular brand of linguistics. The philosophy of LACUS, to put it briefly, is inclusive rather than exclusive. There are plenty of societies around that cater to one brand of linguistic philosophy or another, merely making token allowances for views not shared by the organizers. We have actively sought papers from all theoretical persuasions and will continue to do so in the future. Thus Jean LeGrand of the University of Chicago and Tej Bhatia of the University of Illinois at Urbana presented papers from the Transformational-Generative

point of view; Harold Ekart presented a paper on German modality from the Systemic-Functional point of view, while Carleton Hodge's 'A Set of Postulates for Comparative Linguistics' reminds us of the great achievements and rigor of American structuralism, with the papers in Chapter VII by Lamb, Sullivan, Lockwood, Herrick, Welch, and Babitch offering the insights of the stratificational alternative to both American and European structuralism. Chapter III 'Linguistics as Wissenschaftspolitik' is unique in a special sense; in it, for the first time, the change-over in scientific paradigms is systematically compared to modern politics.

The founding of LACUS and the purport of most of the Forum papers are evidence of the phenomenon I wish to call The Passing of the Syntactic Age, and the dawning of The Pragmatic and Ecological Age in linguistics. The oft quoted 'Chomskyan Revolution' which started in 1957, seems to be running out of steam. It is almost as if the past fifteen years of transformationalism had been but the pilot-wave of a tsunami, the Pacific tidal wave. Before a tsunami hits, there comes a noticeable swelling of the ocean's water level; this sometimes can be so high that boats get thrown onto the shore. Soon thereafter the water recedes exposing corals, crabs, and suffocating fish. In 1948 at the village of Lapahoehoe on the Island of Hawaii, a siren failed to alert the children of a school, and they all ran into the ocean bed picking up the fish and the shells. Soon they were all dead. For the second, or 'killer wave' of the tsunami comes in the wake of the receding water.

I use the analogy of the tsunami to illustrate what probably happened to our thinking in linguistics. Undeniably, something did happen in 1957 when Noam Chomsky published his *Syntactic Structures*; it was the sign of the beginning of changing times. But it was merely the beginning of the rise of the tsunami's pilot wave. It kept rising and rising, and, indeed, some boats were torn off their moorings and thrown ashore. The highwater mark of the pilot-wave occurred probably in 1965 when Chomsky's new book *Aspects* was published. And then came the receding of the waters, leaving the untreated — because untreatable — problems exposed. It was not fish and coral on the ocean floor, but sentences in isolation that were suffocating in contrived linguistic hypostases. Rescue operations began in various parts of the USA: At the University of Chicago James D. McCawley and his students invented 'Generative Semantics' to rescue the paradigm; at Ohio State, and later at Berkeley, Charles Fillmore invented 'Case Grammar'. Indeed, there are usually a few fortunate individuals who manage to carry off a few clams, corals and fish, before the killer wave overwhelms them. There is value in generative semantics and in case

grammar, just as there is in Wallace L. Chafe's semantic encoding machine of 1970. But the tsunami of the cognitive revolution — and we can now see clearly that that is what our tidal wave is all about — was by this time ready to send its killer wave toward the unprotected shores. The killer wave is the recognition that a language is an ecology; more specifically, that people use specific language and dialects to communicate with each other in socially and psychologically, that is pan-anthropologically, describable contexts.

Professor Victor Yngve of the University of Chicago, accordingly, calls his own approach to language 'Human Linguistics'. No linguist who has not read and seriously pondered his proposals deserves any longer to talk about any sort of 'revolution in linguistics'. To date the 'linguistic revolution' in the shallow-past of 1957 is simultaneously myopic and forgetful; additionally its laboriously wrought 'insights' have resulted in an almost total loss of outlook.

Lakes, of course, are not oceans and so physical tidal waves don't really reach us; but lakes are a part of the ecology of the planet and thus function as sensitive resonators of what goes on elsewhere. The coasts have always been more vulnerable both to sudden religious conversions (witness the proliferation of cults in California, and Jehovah's witnesses taking religious dips in the Hudson River) and to technologically inspired fads of make-believe progress. The lake's calmer waters, like a reflecting mirror, would seem to register what is significant and leave out from the chronicle the ephemeral.

We assure all of our members and readers of this volume, that LACUS will never become the counter-pressure group enslaving its members in a philosophy by purporting to liberate them from the paralyzing vibrations emanating from the banks of the Charles River. That liberations are the most liable candidates for totalitarian re-enslavement has been only too well demonstrated and documented both in the political and the intellectual history of this century.

LACUS is a tax-exempt and nonprofit organization incorporated in the State of Illinois. The incorporators of LACUS are: Robert J. Di Pietro (Georgetown University), Jean-Luc Garneau, a native of Québec, (Lake Forest College), John Peter Maher (Northeastern Illinois University), Adam Makkai, a native of Hungary, (University of Illinois at Chicago Circle), Valerie Becker Makkai (University of Illinois at Chicago Circle), and Peter A. Reich (University of Toronto).

Our gratitude goes to Hornbeam Press, Incorporated, and its Director, Professor Richard L. Gunter of the University of South Carolina, who undertook the publishing of this volume; to the

University of Illinois Foundation and its Chicago Circle Campus Director, Miss Susan Sundman for their generous help with transportation for members who could not have come otherwise, and specifically to Mr. Sydney Marovitz and Mr. Thomas Sheffield whose generous gifts made the Foundation's work possible; to the University of Illinois at Chicago Circle's Department of Linguistics which, through the generosity of its Head, Professor Andrew Schiller, made much of the mailing and the organizational work possible and enabled our enthusiastic typist, Kathlyne M. Rog, to receive modest remuneration for her virtuoso performance at the keyboard of the IBM selectric typewriter. Several authors did, however, supply entirely camera-ready manuscripts.

We are launching the *First LACUS Forum* in the hope that both its contents and its physical appearance, as well as the philosophy of the Association, will convince others to join and come to our future meetings.

ADAM MAKKAI

Lake Bluff, Illinois
November 1, 1974

I

The Rehumanization of Linguistics

THE DILEMMA OF CONTEMPORARY LINGUISTICS

Victor H. Yngve
University of Chicago

IN 1937, BRONISLAW MALINOWSKI discussed, in a short book review, the problems facing linguistics as he saw them then, nearly four decades ago. Some of you may know this review as it has been reprinted in an excellent reader on language in culture and society, edited by Dell Hymes. For my remarks today I have taken Malinowski's title, "the dilemma of contemporary linguistics," and his theme. Although I would not embrace Malinowski's position completely, he had much to say in 1937 that is still timely today. Linguistics today truly faces a dilemma, and it is my opinion that the dilemma has reached crisis proportions.

In fact, the dilemma was not a new one in 1937 either, we are told. Exactly half a century earlier, Max Müller's saying "No thoughts without words" had left linguistic studies somewhat in the air. For the psychologist, the aphorism went too far, for obviously symbolic thought without words does occur. Linguistically it did not go far enough, for words mean more than ideas, and in their most important function they are as much a form of human action as any type of bodily behavior. The dilemma came in trying to square the realities of observed linguistic behavior with the traditional views of the relation of language and thought, of words and ideas, of sound and meaning.

Malinowski saw that in 1937 linguists were faced by a similar dilemma. "Once we recognize," he said, "that 'language is a form of activity, a mode of human behavior, perhaps the most important' the question arises: Can we treat language as an independent subject of study? Is there a legitimate science of words alone, of phonetics, grammar, and lexicography? Or must all study of speaking lead to sociological investigation, to the treatment of linguistics as a branch of the general science of culture? If the earliest and most fundamental function of speech is pragmatic--to direct, to control, and to correlate human activities--then obviously no study of speech except within the 'context of situation' is legitimate."

Much the same could be said today. The dilemma today is characterized by a lack of contact between our theories and the results of observation; between grammar and the human linguistic activity observed in everyday life in our own and other cultures. Let us consider, for a moment, some of the many dimensions of this dilemma, some of the ways in which the theoretical structures in linguistics are inadequate as a foundation for our broad observational interests, and how in detail they lack relevance. I would then like to go on and say something about how we got into this dilemma and then perhaps how we might get out of it.

We see today a crumbling of the transformational-generative paradigm. Those linguists who have pinned their hopes on it seem disappointed and disillusioned. Although much interesting work has been done within this framework, it has become more and more evident that it suffers from serious internal problems and may have outlived its usefulness. Even several years ago, in a survey of linguistics from this perspective, edited by Dingwall (1971), nearly every author reflects frustration that the "standard theory" fails to account for many important aspects of natural language, that efforts to patch it up seem *ad hoc*, that the theory by its own criteria is too powerful, and so on. One gets the feeling that work continues in this framework mainly through inertia and for the lack of a more appealing alternative. At major linguistic meetings today one senses a feeling of crisis in the air.

There are other theoretical standpoints besides the transformational-generative one. In fact, for each major theoretician in linguistics, and for many of the minor ones as well, there is a separate theoretical standpoint. It seems that there is no agreed-upon unified body of theory in linguistics--there are many theories, perhaps dozens of them currently, and for each there is a group, small or large, of party-line followers. Linguistics thinks of

itself as a science, yet in a true science such a situation seems preposterous. How can you have a science when no two major theoreticians can agree on the proper shape of linguistic theory? It is a situation more reminiscent of philosophy.

Students are confused. Those who are serious and energetic feel called upon to develop their own version of linguistic theory, and to launch it and try to capture a few converts of their own. The reason they feel impelled to do this is clear: None of the current theoretical positions is really adequate. I can say this even about my own type of left-to-right phrase-structure grammar with discontinuous constructions and subscript conventions that may be familiar to some of you. It would seem that if linguistics is in such a state that the serious student inevitably feels that the theory needs overhauling, our discipline must be in serious trouble indeed.

But it has always been this way! I was amused and not a little shocked in reading a recent book by Ian Michael (1970) on English grammatical categories and the tradition to 1800. He surveys grammatical works written by native speakers of English, which discuss the parts of speech, and were written or printed in England by the year 1800. The earliest is from 1586. This of course leaves out grammars written by Americans and published in America, and so forth, and it leaves out all 19th century and 20th century works. The number of 16th and 17th century grammars in this survey amounts to the astonishing number of 272. What is even more astonishing is that when these grammars are classified just in terms of what parts of speech they recognize, 56 different systems are represented! Going back to the origin of grammar in Greece and Rome, it is still the same story. Each author has a unique system, and each grammar seems to be a law unto itself. Examples may be found in the work by Robins on ancient and medieval grammatical theory in Europe (1951).

Not only is theoretical linguistics in confusion and disarray with its many contradictory versions, none of these versions seems to be adequate to the variety of needs for theory as seen by a wide diversity of linguists who wish to discuss their observations in the light of some appropriate theory. For example, Weinreich, Labov and Herzog, in 1968, discussed the situation from the point of view of the needs for theory in historical linguistics. They pointed out the narrowness and inadequacy of certain theoretical concepts apparently accepted without question by modern transformational linguists such as Noam Chomsky and Morris Halle, and trace their origin to Hermann Paul and the Neogrammarians, inherited through Ferdinand de Saussure and Leonard Bloomfield. They pointed out that the traditional view that language is a homogeneous system and the sole legitimate object of analysis founders when confronted by the empirical evidence of the diversity and inhomogeneity observed in field studies.

Indeed, in sociolinguistics it is widely held, as Hymes has said, that Chomsky's concept of competence leaves the many other aspects of the speaker's tacit knowledge and ability in confusion, and that the contemporary methodological canon of defining linguistic theory as concerned with an ideal speaker-hearer in a perfectly homogeneous community, free from all limitations of actual use, closes off study of the social realities of language by those most able to analyze their linguistic dimension. If the criticism is true of Chomsky's theory, it is perhaps noted only because he has been so explicit on the matter. Most grammatical theories tacitly accept the concepts of language and speech community that imply a uniformity which, in fact, is not an accurate or adequate reflection of the data. Particularly in sociolinguistics one finds variation on a number of sociological and geographic dimensions, and this variation becomes the very phenomenon of interest to the sociolinguist. A theory that assumes uniformity is peculiarly inappropriate.

Similarly, in anthropological linguistics, theories of grammar do not seem to elucidate the relation between language and culture, do not seem to confront the issues of the function of speech in the daily life of the individual and its function in the group and in the society as a whole. These were issues particularly clear to Malinowski.

Grammatical theories again appear to fail us in the field of psycholinguistics. Much of recent psycholinguistics has been an effort to find a psychological reality behind current theories. The conclusion after a large effort in this direction seems to be that there is good evidence against the psychological reality of transformations.

Again in the field of acquisition, we find the grammatically-inspired language acquisition device of Chomsky with its obvious inadequacies. It appears to limit the data that the child uses in acquiring his grammar to the sentences that he hears, replete with errors as they are. The narrowness of linguistic theory leads to ignoring the large influence that the context of situation must have in the linguistic life of the child. The idea that it is a grammar that the child acquires follows similarly from the narrowness of the tradition and would ignore what must be central for the child, the social function of his linguistic activity. It is no wonder that Chomsky has found the rapidity of the child's learning so surprising, and that he has been led to assume such a large innate component. In the area of acquisition, I find the work of Halliday more convincing. Indeed, Malinowski himself seems to understand the essence of what is going on much better than Chomsky does. In the book review I have mentioned, he remarks that: "the only correct treatment is to study the total situation: the vocal act of the child, which is linked up with the circumstances; and the reactions of the adults which respond to the child's prelinguistic activity. The meaning of such a vocal act can only be defined as the change produced by the child's utterance in mobilizing its social environment, and making the adults obey its wishes."

But the grammatical tradition seems completely inadequate to confront the context of situation. In spite of the insightful work of Malinowski, of Firth, of the London School, and of others, we remain, in my estimation, without a theoretical framework adequate for taking into account the context of situation, and of course we must take it into account.

Next let us look at the area of semantics. Language is supposed to be a relation between sound and meaning. Unfortunately we still have little idea of what meaning is. A generation of linguists tried to ignore the problem and do linguistics without taking meaning into account, or at least to reduce to a minimum a reliance on meaning. The hope was that if we worked carefully from the sound end of the relation, one day we would be able to work out a satisfactory semantic theory. So far it has not happened, and this is just another one of the many reasons that linguists feel impelled to revise and rework the theoretical structures of our discipline.

Some philosophers have set up a trichotomy between syntax, semantics, and pragmatics. Recently, linguists have become concerned with pragmatics--actually it is not so recent if one reads Malinowski, who was saying in 1937 that the earliest and most fundamental function of speech is pragmatic--to direct, to control, and to correlate human activities. But as recent work has shown, there are still many problems in this area, theoretical problems that our theories seem inadequate to confront.

Now let us think for a moment about the restriction of grammar and language theories to speech, and consider the problem involved in accounting also for the kinesic components of communicative behavior, and the important relevance of distance and space as studied in proxemics. How can we find a way to embrace these phenomena in conjunction with a grammatical theory of linguistics? It is a question that sets one wondering. It mirrors an inadequacy that is quite serious, although it is widely ignored by grammarians. Our basic theories should not serve to equip us with built-in blinders so we may ignore these many problems.

It is a long and dreary list, this recitation of the inadequacies of contemporary linguistic theory, and it is incomplete. I could continue for some time with other inadequacies of the theory in confronting observational evidence, and after that mention its inadequacies in applied fields such as rhetoric, the teaching of foreign languages, the treatment of style, and the understanding of

poetic language. I could discuss the recurrent complaints of the teachers of English that grammar seems irrelevant to the students, and often to the teachers as well. Instead I will turn to some of my own experiences.

SOME YEARS AGO I became engaged in an effort to find out how to translate languages by computer. Almost from the very beginning of this effort, before 1950, it was evident that the linguistic difficulties standing in the way of the development of translating machines would be more serious than the technical computer difficulties. This was recognized in the writings of such pioneers as Erwin Reifler, Victor Oswald, and Yehoshua Bar-Hillel.

What we were in effect asking was that the computer would translate by applying an elaborate set of rules based on lexicon and grammar. It was hoped that the results would be translating programs capable of producing a useful product, and that the neglect of more subtle semantic and stylistic issues, for example, would not unduly degrade the quality of the translations produced. Of course the machine would never know or understand what it was translating, as a good human translator does. It was thought that to produce a machine that could understand would be vastly more difficult, and unreasonable to ask as a prerequisite to a translating machine. It was realized that the writing of routines to produce translations by the automatic application of rules would not be easy. We would certainly need the best knowledge that linguistics had to offer as a scientific foundation for the proposed application.

The concept of a machine that would produce translational equivalences by rule without knowing the meaning was quite consonant with the best linguistic knowledge of the time. Descriptive linguistics had its eye on form and structure. In its effort to be rigorous and scientific, it tried to rely entirely on its text or corpus as data; it greatly mistrusted any reliance on meaning, or eschewed it altogether. Nevertheless it attempted to isolate and study the meaning-bearing elements found in utterances. Bloomfield had defined morphemes and tagmemes as the smallest meaningful units of phonetic and grammatical form. He operated with the explicit principle or assumption "that a language can convey only such meanings as are attached to some formal feature: the speakers can signal only by means of signals." If meanings were signalled by formal elements of discourse, the way seemed clear to devise routines for recognizing and extracting the meaning-bearing elements of any particular input sentence in terms of the formal features described in a linguistic analysis of the language, and to translate them by selecting the appropriate elements, features, or signals of the output language and building them into an appropriate output sentence.

We envisioned a three-step process by which sentences would be translated (Yngve 1957, 1967). The first step would analyze the incoming text and determine its grammatical and syntactic structure. The second step would convert the structural descriptions of the input sentences into translationally equivalent descriptions in terms of the different categories and structures of the output language. The third step would then produce to order output sentences answering to these descriptions. We found it easy to solve the computer-related problems in this scheme. This left the linguistically-related problems.

What was needed was a linguistic metalanguage or notation system in which we could write descriptive grammars of a degree of explicitness and completeness never before attempted. An adequate notation for this purpose did not exist and a search for one was instituted. Our attention was attracted to the new phrase-structure and transformational notation of Noam Chomsky, which involved a linguistic process of rule application resembling the computer processes of symbol manipulation. We were also intrigued by the power of transformations and the promise they held out for an eventual approach to semantics. I was perhaps one of the first persons to become excited about transformational grammar, and one of the first to become disillusioned with it.

The problem in this instance was that the notation mirrored a theory of grammar in which the sentences of a language were generated all at once, as it were, whereas our recognition routine and construction routine would need to deal with sentences one at a time. It seemed a small difference, and we spent considerable time trying to modify the notation and theory so that it would serve our application and still retain the transformations. We were unsuccessful in the effort and reluctantly came to the conclusion that some of the most basic tenets of transformational grammar were incompatible with the needs of a mechanizable grammar of the sort we needed. The constraint that our grammars must specify actual real-time processes of sentence construction was unreconcilable with the type of linguistic abstraction embedded in Chomsky's theory of generative grammar. Even today linguists generally are as surprised to learn this as we were. Chomsky's form of grammar was completely unsuited to the function of directing the operation of computer routines for sentence analysis or sentence synthesis. If it had ended there, I would not be telling you this. Perhaps a theory of grammar should not be faulted if it is unsuitable for operation in conjunction with computer programs for analysis or synthesis, an application for which it presumably was not designed. But there is more to the story.

IN SPITE OF MUCH loose talk by generations of linguists about people using language, Chomsky, following the tradition in its orientation to static text or corpus, had abstracted away from the use of language. In so doing he had not developed any conceptual structure or theory relevant to handling the fact that speakers and hearers operate in real time and do one thing after another. This is an issue that was soon raised in our work, that is, the issue of what people do, and this is an area where Chomsky had made some vague claims of relevance for his theories. He would like to say that people have transformational grammars in their heads, and that the transformational apparatus is innate in the child.

What we turned to instead for our computer application was an immediate-constituent model of sentence structure of the type that had been especially well explored during the preceding twenty-five years by such men as Bloomfield, Wells, Nida, Fries, and Harris. Although Chomsky had argued that phrase-structure alone was inadequate for a linguistic theory, we were able to modify the phrase-structure notation and theory for our purposes and we did not in fact find it inadequate. Our computer model for sentence production utilized a left-to-right phrase-structure grammar with unordered rules. It produced grammatical sentences a word at a time in their proper order.

The sentence producing routine operated with a temporary memory to hold grammatical commitments. That is, when embarking on a construction of two constituents, it would work on the first constituent first, holding in a temporary memory during this time a complex symbol representing the grammatical and syntactic features necessary to ensure that the second constituent would be of the proper type to stand in construction with the first.

Now since constructions can be nested inside of constructions, it would often be the case that several constructions would be open at the same time, so that there would need to be stored several complex symbols for remembering how to close each of the open constructions. These complex symbols were held in a temporary memory of the pushdown type. A limit on the capacity of this temporary memory to, say, seven complex symbols corresponds to a limit of seven grammatical constructions open at any one time. Now on the basis of the left-to-right and top-to-bottom nature of the procedure for constructing sentences by this model, this leads to a limit of seven in what I called the depth of the sentence produced, and what Chomsky has called left-branching.

If we take this computer model seriously as a model of a speaker, the question arises as to how a limit of only seven simultaneously open constructions could be enforced. It was proposed that the enforcing would be done mainly by the grammar, and only rarely would a processing fault occur during speech on account of memory overflow. The grammar would be built so as to be

well-behaved according to certain structural principles. It would almost never present the speaker with the grammatical possibility of having more than seven constructions open at once. But to the extent that the grammar was not perfect in this regard, the speaker might occasionally run into difficulties. Such difficulties would represent a force for changing the grammar so as to reduce the incidence of such difficulties.

Theoretical analysis of the behavior of the grammatical model led to a number of structural principles that together would tend to ensure that a grammar would be well-behaved and not allow more than about seven constructions to be open at once. A synchronic analysis of English syntax then revealed that all of these structural principles were actually found to be operative in English, and that, indeed, much of the complexity of English syntax could be explained on the basis of this hypothesis (Yngve 1960). The following idiosyncrasies of English structure can be easily understood in the light of the hypothesis: the hierarchy of sentence, clause, noun phrase, adjective, and adverb; the different behavior of subject and object clauses from the point of view of clauses within clauses; the phrase structure of the active and the passive with a *by* phrase; the reversal of order of direct and indirect object when one or the other is a pronoun or is extensively modified; the shifting of the position of the separable verb particle either before or after the object; as in *He called up the girl, He called the girl up;* the function of the anticipatory *it* in sentences like *It is true that he saw her;* the first position of the interrogative pronoun; the position of the adjective before the noun and the relative clause after the noun; the discontinuous nature of certain adjectival and adverbial phrases; the position of certain adverbs before the article, as *certainly not a very clearly defined color;* the fact that when the genitive marker follows its noun phrase it is a suffix, *-s*, and when it precedes, it is a separate word, *of*; and that derivational affixes are suffixes, and prepositions, articles, and conjunctions are separate words. This depth hypothesis thus rests both on a theoretical analysis of the linguistic model, and also on the observed synchronic evidence of the existence of the predicted structures in English.

Although the grammatical model involved in this depth hypothesis was perceived as a rival grammatical theory by Chomsky, and opposed as such, the fact remains that the predictions of the depth hypothesis, and the insights it affords into the *raison d'être* of many of the otherwise perplexing features of English syntax could not and can not be understood from the point of view of a generative theory. It takes a procedural theory that models what people do. And although, as I have already said, the grammatical model used in our computer application, which led directly to the depth hypothesis, is inadequate as a complete linguistic theory, there is today no adequate linguistic theory. It still remains the best theory we have for considering the effect of a small temporary memory on speech production and on language structure.

THE NEXT THING THAT HAPPENED was Chomsky's retreat to competence. He could not confront the phenomena related to temporary memory in his theory, which was not procedural. He professed not to be interested in matters of mere performance. But the fact remains that the depth hypothesis does make claims about how a grammar must be structured in order to operate properly in conjunction with part of the innate equipment of the speaker, that is, his limited temporary memory. The way that Chomsky was able to admit the essential truth of the depth hypothesis while appearing to reject it altogether is a revealing study in linguistic polemics, and a sad commentary on the state of linguistics when grammatical theories must be defended on irrelevant grounds. Actually, come to think of it, how are we to decide between rival grammatical theories? In the light of the large number of theories, both extant and extinct, this is a serious problem in the conceptual foundations of our science.

So the problem of mechanical translation remains unsolved. No grammatical theories appear to be adequate to this task. The biggest difficulties

appeared to be in the area of semantics. It began to become clear that the machine would only be able to translate adequately when it could understand the meaning of what it was translating, and in spite of the fact that grammar is supposed to map the relation between sound and meaning, no one to this day seems to know what meaning is in any concrete way that would make possible the automation of translation via the meaning.

The unexpected success of the grammatical theory we developed, in giving insight into the complexities of syntax, and into the relation of these complexities to a limited temporary memory, was a by-product of the computer-related research. These results can be attributed to the limited extent to which we tried to make the computer program model what people do in actually processing linguistic information and actually constructing sentences.

The hypothesis made some very explicit claims about historical language change. It laid out the structural conditions that would lead on the one hand to compounding, suffixation, prefixation, and agglutination; and on the other hand to the exact structural conditions that would lead to the characteristics of analytic or isolating languages. The proposed mechanism of change involved psycholinguistic factors in speech production, related to the limited temporary memory of the speaker, and the growth in the language first of stylistic constraints of awkwardness and then grammatical constraints.

These proposals were put forward as an hypothesis in the hope that historical linguists would test them. But as far as I know they have not done so in the nearly 15 years that have elapsed. Recently, an expert in the history of English called my attention to the work of Charles Fries and his students, published back in 1938 and 1940, on the history of the English genitive. Since Fries' work seems to bear out the predictions of the hypothesis in detail, I was moved to examine the matter myself, and to discuss at the recent LSA Summer Meeting how the hypothesis applies to the findings of Fries, and how it provides them with an explanation.

Briefly, what Fries and his students did was to count occurrences of genitive constructions in texts from Old English and Early Middle English. They found that in 900 A.D. the inflected genitive appeared in equal numbers either before or after the modified noun. But in the 300 years between 900 and 1200 A.D. the proportion of preposed inflected genitives grew to outnumber the postposed ones by seven to one. This parallels the leveling and loss of inflections and is an example of the growth of the word-order pattern of modifier before noun. At this point the periphrastic genitive with *of*, which had earlier amounted to only about 1% of all genitives, grew very rapidly and reached 85% by 1300 A.D., replacing all of the postposed inflected genitives and many of the preposed ones. Inflected noun genitives have been further replaced by uninflected adjunct nouns as *the door handle*, *the summer months*, which feed into the building of noun compounds such as *doorknob*. For some reason the pronoun genitives were able to resist the pressure of the very rapid rise of the periphrastic genitive. They remain predominantly in the preposed position. These facts can all be explained in detail in terms of the aforementioned hypothesis.

Thus the depth hypothesis has met with partial confirmation in respect to its predictions in historical linguistics. Further work of testing needs to be done. What concerns us here is the question of why a grammar model which seems to be unsatisfactory as a complete theory for linguistics also seems to provide insights and predictions that square with historical evidence that was not taken into consideration when the model was devised. A related question concerns why it is that no other grammar model is capable of giving these results and predictions. I have been concerned with these questions for a number of years. The failures and successes of this grammatical theory hold a clue to what might be done to resolve the dilemma facing contemporary linguistics.

INSTEAD OF LOOKING at the many hotly contested differences between the dif-

ferent existing grammatical theories, perhaps we should look at their similarities. Since no grammatical theory makes adequate contact with the results of observation in a number of broad areas such as sociolinguistics, psycholinguistics, anthropological linguistics, and so on, perhaps the lack can be attributed to what they have in common. We thus turn first to the most basic assumptions underlying linguistics as a discipline.

Linguistics is usually defined as the scientific study of language. Language is conceived as a relation between sound and meaning, captured in terms of grammar and lexicon, which provide an autonomous theory of language and serve to separate the grammatical utterances of the language from the ungrammatical. The data to be accounted for are in the form of text or corpus. Language expresses meanings, which represent the logical or propositional content of the sign relation between sound and meaning. Grammatical utterances are independent of the use to which they are put, and independent of the situational context in which they are uttered.

That is a brief summary of what most grammatical theories have in common. I have come to realize that every one of the above basic tenets is open to serious question and should be discarded. I have made these arguments in a book that is nearing completion.[1] Many of them have been made separately in other contexts. For example, I have already mentioned that Weinreich, Labov and Herzog, from the point of view of the needs for theory in historical linguistics, have pointed out the narrowness and inadequacy of certain theoretical concepts apparently accepted without question by modern transformationalists, and trace their origin to Hermann Paul and the Neogrammarians, inherited through Ferdinand de Saussure and Leonard Bloomfield. They are referring to the traditional view implicit in the above paragraph that language is a homogeneous system and the sole legitimate object of analysis in linguistics. Malinowski was making much the same point when he said: "The distinction between *language* and *speech*, still supported by such writers as Bühler and Gardiner, but dating back to de Saussure and Wegener, will have to be dropped. Language cannot remain an independent and self-contained subject of study . . . The dilemma of contemporary linguistics has important implications. It really means the decision as to whether the science of language will become primarily an empirical study, carried out on living human beings within the context of their practical activities, or whether it will remain largely confined to deductive arguments, consisting of speculation based on written or printed evidence alone." Today we hear complaints that Chomsky's concept of competence must be abandoned. The point is much the same. Chomsky has several times said that by pushing a precise, but inadequate formulation to an unacceptable conclusion, we can often expose the exact source of this inadequacy. It seems to me that a great merit of Chomsky's work is that he has tried to make very precise the assumptions underlying traditional grammatical theory, and by so doing, he has exposed them to our view and now we can see their inadequacies.

Adam Makkai asked me the other day, "Where did we go wrong?" I think it goes farther back than Saussure or the Neogrammarians. It goes back to the very origin of grammatical theory in Greece, in the second and third centuries B.C. Pretty much the whole position that I cited above was developed explicitly at that time. There were, as you know, two threads, one the Alexandrian concern for literary criticism, a rejuvenation of poetry and the pure language of the ancients; the other the philosophical concerns of the Stoics for a theory of knowledge. These two threads came together and gave us grammar, which was cultivated through two millenia in the service of maintaining standard literary languages. The philosophical content, and the fundamental assumptions of linguistics, we owe mainly to the Stoics. Its preservation and current force we owe to the demands for prescriptive grammars.

Linguistics has long ago renounced a prescriptive goal and has turned

instead to a scientific study. What has perhaps not been realized is that even while abandoning prescriptivism, we have retained the traditional grammatical concepts fostered and honed in the prescriptivist tradition for over 2000 years. The source of the concept of an ideal speaker-hearer in a perfectly homogeneous speech community is the prescriptivist concept of a person with a perfect command of a standard language. The source of the idea that language is a relation between sound and meaning is the Stoic philosophical concept of a theory of knowledge which addressed the question of how do we know the truth and how do we express it. The Stoic grammar looks surprisingly modern. It had levels or components justified on the basis of well-formedness and the possibility of structural ambiguity. The Stoics had a logical deep structure called lekton, and their logic was greatly advanced over Aristotle, resembling more the modern theories of Frege and Carnap. The idea that grammar is independent of its use fits with the prescriptivist idea that the function of grammar is not to tell you what to say, but how to say it correctly. The idea that language is independent of the context of situation fits with the requirements of a theory of knowledge concerned with eternal truths. Certainly in such a theory an expression of an eternal truth should be completely independent of the context of situation. I have reluctantly concluded that we must go the whole way, and discard this hoary tradition. Many others have called for abandoning one or more parts of it, but to no avail. The pressure of the tradition has been too great. It is ingrained not only in the grammatical tradition and the basic assumptions of our discipline, but also in our everyday language, to the extent that the whole bundle of philosophical preconceptions have taken on the aura of self-obvious truths.

Linguistics is supposed to be a science. The history of the sciences showsthat one by one they have crystallized out of philosophy. One by one they have found their unique objects of study in the phenomena of the external world, and have abandoned philosophical speculation and philosophical preoccupations. It is surely time for linguistics to finally cut the ties with philosophy and become a true science. Of course we do not want to throw the baby out with the bath water. Much has been discovered in the last 2500 years that we will not want to forget. But at the same time, we do not have to relate our knowledge to an outmoded philosophical position that we have inherited mainly via the prescriptivist grammatical tradition. We will want to restructure our knowledge in a new framework, hopefully one fully adequate for the much broader interests of linguists in the 20th century--the interests of the sociolinguists, the psycholinguists, the anthropological linguists; the interests in kinesics and proxemics; the interests in the functions of speech; in what people do with words and how they do it. Such a complete theoretical overhaul will not be easy; it is not something that can be done in a few months, like devising a new kind of grammar. That's just the point, it is not just devising a new type of grammar. We have too many types of grammar already, and they are all inadequate. What has to be done is a rebuilding at a much more basic level, and I do not expect that it will be easy. But I do have some ideas as to how it might be done. They involve carrying out the program implicit in some of the things that Malinowski has said, and what many others have also said.

THE CENTRAL DIFFICULTY with the tradition is the very concept language, the purported object of study in linguistics. It is not an adequate concept around which to form a discipline capable of approaching the kind of questions that linguists are asking today. The main reason is that it does not focus on people. If I were asked to give a concise statement of the dilemma of contemporary linguistics, I would say it is the question of whether we should study language or people. Malinowski was most explicit in saying that we should abandon the study of language. He was less explicit as to what we should substitute, sometimes saying we should study people--in his words "an empirical

study, carried out on living human beings within the context of their practical activities," sometimes advocating that we should study speech and not separate it from the study of language.

If linguistics is not to be the scientific study of language, what should it be? The proper statement of our goal is a matter of the greatest importance. If we can center it somehow on people, we may be able to design a linguistics that can confront with its theories the diverse observations and data that we have been talking about.[2]

I should like to suggest that the goal of linguistics be *to achieve a scientific understanding of how people communicate.* Such a goal, I believe, does have the appropriate coverage and scope.

It is in the nature of a goal that it directs our choice of paths in trying to reach it. It is therefore important to consider carefully the exact wording of the goal. We would do well to avoid the use of terms that appear to prejudge the approach or limit the means for finding the answer. As an example of the danger, a previous wording of a goal to cover the broad scope of linguistics (Yngve 1969) was to achieve a scientific understanding of how people use language to communicate. Despite expressed intentions to the contrary, this formulation has the same problem that the current goal of linguistics has, that is to achieve a scientific understanding of language. The use of the term "language" appears to presume part of the answer in that it implies close ties to the grammatical tradition. Another inappropriate suggestion would be to achieve a scientific understanding of communication. This would bring the presumption that there is some abstract entity called communication that is appropriate as an object of study, but then we would be left with the task of defining communication, and this could lead to confusions as great as with the presumption of an abstract entity called language.

Our chosen wording, "to achieve a scientific understanding of how people communicate," does not seem to suffer from any of these defects. There should be no problem with the phrase "achieve a scientific understanding," for a limitation to a scientific approach seems appropriate. The term "people" can be taken in its ordinary meaning in English. Its use merely carries the reasonable presumption that there are people. The only possible problem is with the verb "communicate"; we did have a problem before with "communication." But whereas the nominalization is vague, even ambiguous, presuming some kind of entity or abstraction, the verb does not share this problem. The only presumption is that people communicate. But again taking the ordinary meaning of the word, we know from observation that people do communicate. This statement of the goal thus simply points to an interesting observable phenomenon, that people communicate, and proposes to achieve a scientific understanding of it. The scope of this goal should be approximately if not exactly the same as the observational scope of the various broad interests of linguists.

I have called the discipline answering to this goal *human linguistics.* This is to eliminate any confusion with the traditional linguistics of language, but at the same time to recognize that the interests of linguists today probably come closer to this goal than the interests of any other current sizable group of scholars, and furthermore, linguistics has probably already given us more insights into this area than any other discipline.

IT IS A GREAT ADVANTAGE of our new goal that we can start with people. An appropriate scientific concept of people for the purposes of human linguistics should not be difficult to formulate, for people are objects that are given in advance and that can be considered from different viewpoints: there are several other sciences that study people. Since a person is a real physical object that can be known from points of view other than human linguistics, the concept of person does not suffer from the difficulties associated with the concept of language or the concept of communication.

Of course, we are not interested in studying every aspect of people, but only those aspects that are related to how they communicate. It is thus

appropriate that we set up as our first concept in human linguistics an abstraction that includes just those properties of people that are of interest. We define the *communicating individual* as an abstraction in linguistics that includes just those properties of the person that are required to account for his communicative behavior. In defining the communicating individual in terms of properties of interest in human linguistics, we simply exclude from consideration that which is irrelevant while at the same time maintaining the flexibility required to accommodate new observations about people that can be related to their communicative behavior.

The communicating individual as thus defined becomes the central concept in human linguistics and represents its major object of study. This being the case, it is clear from the very beginning that human linguistics is a part of psychology. The relationship of the traditional linguistics of language to psychology has been far from clear and has caused no end of confusion in the literature.

Communication is a group or social phenomenon, and it would seem that we would need in human linguistics at least one concept related to groups of individuals. But what kind of a concept should this be? One such concept that has been used extensively in linguistics is the concept of the speech-community. This concept is usually defined in such a way that the members of a speech-community are those people who speak the same language. The concept has given much trouble and has been discussed extensively in the literature. For our purposes it is inadequate because it is related to a concept of language which we have rejected as a basic concept in human linguistics.

We have defined the communicating individual on the basis of observable phenomena--that there are people and that they communicate. For setting up a concept of groups of individuals in human linguistics we look again to observable phenomena for guidance. We observe that there are many people in the world but that no two are exactly alike in their communicative behavior. Yet we do observe groups within which the individuals can easily communicate with each other. We must somehow take into account both the similarity and the diversity of individuals. With a focus on people instead of on language, we are in a much better position to do this. Being free of the tyranny of the concept of language, we no longer have any reason to conceive of an individual as a language-speaker. We are then not led to an unrealistic assumption of homogeneous communities of language-speakers. We can define our groups in ways that properly reflect the facts of communication.

An individual obviously belongs to many different groups at the same time. We can accommodate the observed diversity of communicative behavior by taking these different groups into account, and thus solve a number of problems that concepts of speech-community have raised. We define a *colingual community* as a group of individuals who can communicate with each other in certain ways characteristic of the group. An individual is normally a member of many different and partly overlapping colingual communities. Each colingual community is characterized by one or more linguistic features or items that serve to define the community and to set it apart from the others. The concept of colingual community is not related to the totality of communicative potentialities of the individual, but only to those that are characteristic of that community and serve to distinguish it from other colingual communities.

When individuals engage in communicative activity, they will be expected to make use of linguistic features belonging to several of the colingual communities to which they both belong. Thus a member of a profession would belong to a colingual community distinguished by certain communicative forms such as vocabulary items. When he communicates with other members of this community, he may make use of these forms. But he will also make use of linguistic features characteristic of other colingual communities. These will of course include larger communities with a geographic base as well as smaller or overlapping ones related to such special interests as hobbies, sports, religion, fraternal organizations--and even to intimate family groups, as Sapir once emphasized.

Colingual communities are thus defined in terms of people and their observable behavioral properties related to communication. It would seem that all of the phenomena previously associated with the similarities and differences of languages, dialects, jargons, technical languages and the like can be analyzed, described, and understood in terms of colingual communities and the communicative features used to define the communities and to identify individuals as members of them.

We have defined the communicating individual as an abstraction in linguistics that includes just those properties of the person that are required to account for his communicative behavior. We may call these properties *linguistic properties* and note that by definition they account for all the properties of the communicating individual, these being a subset of the properties of the person. An investigation of linguistic properties will be a major concern of human linguistics since the communicating individual is its central concept.

There are various ways in which linguistic properties may be classified. One way is to divide them into *innate properties* and *acquired properties*. This method of classification runs into the well-known problem that a mature individual is a product of both heredity and environment, and for any given observable property there may be both hereditary and environmental contributing factors. For example, the characteristics of the voice of an opera singer could probably be traced to both genetic factors and training, and it may be impossible to separate these two essential components in the observed singing voice. Nevertheless the classification into innate properties and acquired properties does have some merit since their methods of study are somewhat different. For studying innate properties we have, for example, methods worked out in genetics, and for studying acquired properties we have, for example, methods worked out in developmental and educational psychology.

It would seem far off the mark to approach the innate properties of the communicating individual through linguistic universals. Grammar is not a property of a person but a construct of the grammarian that purports to show the relation between sound and meaning given by some language. The idea of universal grammar as including just what is common to the grammars of all languages is an interesting abstraction in the linguistics of language, but to represent this as being an innate property of the individual characteristic of the species is to make a category mistake of the most glaring kind.

Care must also be exercised, when studying the innate properties of the individual, not to assume that just because an observed property is thought to be universal (that is, possessed by all individuals), it is therefore shown to be innate. It is quite possible that there are universal communicative requirements that would be realized in all communicating individuals through acquired properties. For example, one might expect all individuals to have a means of communicating assent or dissent, or for selectively addressing another individual in a group. Such properties might well be universal, yet acquired.

Another way of classifying linguistic properties is according to their apparent function in communication. A full understanding of the communicative function of various linguistic properties will, of course, have to await the results of a considerable amount of research. We can, however, suggest a tripartite classification that is based on some initial observational evidence. Linguistic properties can be classified as *basic properties*, which represent the physical properties that the individual uses in communication; *colingual properties*, which represent those properties by virtue of which the individual is a member of various colingual communities; and *situational properties*, which represent the individual's changing internal accommodation to the communicative situation.

The *basic properties* of the communicating individual are probably largely innate, but there may be a non-negligible acquired component. They represent the physical mechanism, the basic equipment, that the individual uses in

communicating. Examples include the so-called organs of speech, the equipment for hearing, arms and hands to gesture with, facial features, and a nervous system and brain. In human linguistics we consider these basic properties from the point of view of their communicative function. Included among the basic properties are a large permanent memory and a small temporary memory capable of holding about seven items, as previously mentioned. It is not the task of human linguistics to discover the physiological mechanisms of nerve conduction, but eventually all the basic linguistic properties may be understood also at a lower biological level in terms of anatomy and physiology, and then ultimately understood by scientific reduction in terms of biochemistry and physics. This possibility of reduction to levels of explanation in the biological and physical sciences is one of the advantages of human linguistics over the linguistics of language. Reductions of this sort strengthen both the higher level science and the lower level science.

The *colingual properties* of the communicating individual are those acquired linguistic properties by virtue of which the individual is a member of various colingual communities. Such properties are the basis for generalizations about communicative behavior because they represent aspects of communicative behavior characteristic of groups of individuals. Thus we have a wider observational base than the single individual for the study of colingual properties, yet they are properties of the single individual. The colingual properties include a knowledge of the communicative relevance of various roles that are recognized in certain colingual communities, and how to act and react appropriately according to these roles. They are those acquired properties by which the individual knows what to do or say or what to understand in various communicative situations. Thus under the colingual properties of the individual we understand much of what would be covered under language, or under various cultural or social concepts, but from a quite different point of view that allows us to have an integrated framework.

The *situational properties* of the communicating individual are those acquired and changing properties by which he accommodates to the changing communicatively relevant situations in which he finds himself. Of course, the external physical environment may have communicative relevance, but it is more appropriate to take this into account only through information residing in the individual about the external physical environment. The superiority of this view can be easily shown: when the individual has a misapprehension about his environment, it is his misapprehension and not the reality of the environment that controls or governs his communicative behavior. Under situational properties we include not only a knowledge of the physical situation, objects and persons in the vicinity, their names, roles, characteristics, and so on, but also more abstract situational factors such as the topic of conversation, knowledge of common experiences, attitudes, wishes, hopes and aspirations, knowledge of events--current and historical--and many other pieces of knowledge that have communicative relevance. Both colingual and situational properties, though acquired and perhaps temporary, have their basis in real biochemical differences in the individual; and at the psychological level, an individual who knows something of communicative relevance is demonstrably different from a person who doesn't, or who knows something different.

The basic, colingual, and situational properties of the communicating individual are exhaustive, and together they can completely account for the communicative behavior of the individual. The basic properties cover the mainly inherited mechanisms operative in the individual for communication; the colingual properties cover the knowledge that the individual has of what to do or understand in various communicative situations; and the situational properties provide the individual with a knowledge of the operative situations. Human linguistics can thus proceed with the study and elaboration of four structures: the structures of the basic, colingual, and situational properties, and the structure of their interrelation. These are structures of real physical properties of the individual considered from their communicative

function. Since we are studying in human linguistics an aspect of physical reality, we can ask meaningful questions that can be approached through observational and experimental methods.

PERHAPS AN EXAMPLE WOULD HELP to clarify these basic concepts. Suppose I meet my friend John on the street, invite him to my house for the evening, and he accepts. Much is known about interactions such as this from the work of a number of perceptive observers. How would this bit of face-to-face behavior be approached from the point of view of human linguistics?

As I walk along the street, I am aware of a figure approaching and set my course so as to avoid a collision. Now the behavior involved in foot navigation and the avoidance of obstacles shades into noncommunicative behavior. But the fact that I am a person who passes on the right is a colingual property. But I can adapt in cities where they pass on the left, so my knowledge of which is currently appropriate is a situational property. I note that he has also moved so as to pass on the right. Thus I am aware of being in an impending right-passing situation, and my situational properties change. I become a person who is about to execute right-passing behavior, and if the oncoming person should change his course I may be caught off guard.

I also know I am in a neighborhood where I may meet a friend--a situational property--and I know how to handle head position and direction of gaze up to the point of recognition--a colingual property. When recognition takes place, the situation has changed and I behave appropriately. I will do this even in the case of mistaken recognition, until I realize my mistake. The situational properties now are that I recognize my friend, I remember that his name is John, and I have available certain relevant information about our relationship. It is also a situational property that I choose to stop and speak with him rather than execute the minimal appropriate greeting and pass on.

I know how to do this: how to indicate that I wish him to stop, how to engage him in conversation, and he knows how to react appropriately to my behavioral cues. These are colingual properties. I know how to ask him over for the evening. I know that to do this I must be in conversation with him and must have the conversational turn. I do all this and the situation has changed. He becomes a person who is in conversation with his friend, has just been asked over to the friend's house for the evening, and now has the conversational turn. These are his relevant situation properties, which, together with other situational properties, will determine whether he accepts or not.

He knows what the options are as part of his colingual properties. He decides to accept and knows how to do that. He does so. He is now a different person. His situational properties have changed again. He is a person who has been invited over for the evening and has accepted. I can predict with some degree of certainty that he will show up on my doorstep. And I am a different person, too. My situational properties are different. I now expect him to show up.

NEEDLESS TO SAY WE ARE VERY FAR from a complete understanding of the structure of the communicating individual in terms of his basic, colingual, and situational properties, and in this brief paper I have not been able to treat fully what is already known. Many questions have been raised that can be decided only by observation and experiment, and much needs to be done to work out the details of these structures.

It would seem premature at this point to try to specify more exactly the abstract or formal nature of these properties of the individual: we do not yet have enough observational evidence. There is a danger in the premature adoption of a formalism: we may get overly committed to an inadequate framework that would constrain further research. Witness the problems that the

grammatical tradition has caused in the study of communicative phenomena.

Nevertheless there are indications that some kind of a state theory of the individual would be appropriate (Yngve 1969). There are many formal state theories available, but I feel we do not yet have enough observational evidence to choose between them. I have several pieces of research that tend to make me favor a state theory of the individual. The first is the work done on the depth hypothesis. Although this work was strongly influenced by concepts from the linguistics of language, with its emphasis on the sentence, it did succeed in relating a basic and undoubtedly innate property of the individual, that of a limited temporary memory, to observed syntactic structures in language and to language change. Whatever success this work has can be traced in large part to the type of state theory postulated for the individual as a producer of sentences. It is possible to recognize in that model the precursors of the basic, colingual, and situational properties of human linguistics. The other relevant pieces of research are more recent, both reported at the Chicago Linguistic Society. One (Yngve 1970) reported work on conversational turn and was entirely motivated by considerations of a state theory. The other (Yngve 1973) reports further work on the relation of temporary memory to conversational behavior. It was an attempt to observe what would happen if a speaker's temporary memory were overloaded by an extraneous task. It proved possible to produce experimental conditions where people would forget what they were going to say. I am not sure, however, if this phenomenon is at all related to the disfunction postulated as part of the depth hypotheis.

What can we now say about the dilemma facing contemporary linguistics? Let us look again at the questions that Malinowski asked, and compare the answers that he gave to the answers proposed here. When he said: "language is a form of activity, a mode of human behavior, perhaps the most important," we would agree with him, only quibbling about terminology in his use of the term language. When he asks, "Can we treat language as an independent subject of study? Is there a legitimate science of words alone, of phonetics, grammar and lexicography?" we would agree with his answer: "Language cannot remain an independent and self-contained subject of study." When he asks, "must all study of speaking lead to sociological investigation, to the treatment of linguistics as a branch of the general science of culture?" we must answer "no," for the concept of culture is an abstraction cut from the same cloth as the concept of language. I do not recommend that we study invented abstractions. They have a certain arbitrariness and indeterminancy that can lead to a multiplicity of different theories, all inadequate to confront the observable phenomena. We would do better to adopt a more concrete object of study such as people, and take as the goal of linguistics the scientific study of how people communicate. If we do this, in the way that I have suggested, or perhaps in some better way, linguistics will finally be able to develop a unifying theory capable of embracing the vast range of phenomena of interest to various linguists: phenomena in anthropological linguistics, sociolinguistics, dialectology, psycholinguistics, historical linguistics, acquisition by children, communication in groups, and so on. And with such a linguistics, the way will be open to more fruitful applications in teaching, in automation, and even in the preparation of superior normative materials, still a legitimate demand in many communities.

Footnotes

1. To be entitled *An introduction to human linguistics.*

2. The remaining paragraphs, except for the last two, are a lightly edited version of a portion of my remarks addressed to The IXth International Congress of Anthropological and Ethnological Sciences (Session on the organization of behavior in face-to-face interaction) August 28 to September 8, 1973, Chicago Illinois, and in press in the proceedings to be published by Mouton.

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LANGUAGE AS SOCIAL SEMIOTIC: TOWARDS A GENERAL SOCIOLINGUISTIC THEORY*

M.A.K. Halliday
University of Illinois at Chicago Circle

(0) Introductory

Probably the most significant feature of linguistics in the seventies is that man has come back into the centre of the picture. As a species, of course, he was always there: his brain, so the argument ran, has evolved in a certain way-*ergo*, he can talk. But truly speaking man does not talk; *men* talk. People talk to each other; and it is this aspect of man's humanity, largely neglected in the dominant linguistics of the sixties, that has emerged to claim attention once more.

Linguistics is a necessary part of the study of people in their environment; and their environment consists, first and foremost, of other people. Man's ecology is primarily a social ecology, one which defines him as 'social man'; and we cannot understand about social man if we do not understand about language. In order to suggest this perspective, linguists came to talk of 'sociolinguistics'; and this term has been repeatedly discussed and evaluated in relation to various quasi-synonyms such as 'sociological linguistics', proposed by Firth (1935), 'institutional linguistics' (Hill, 1958) and 'sociology of language' (Fishman, 1967). There is some consensus to the effect that the study of language in its social context is simply an aspect or facet of linguistics, while anything that is interpreted as significant covariation between linguistic and sociological phenomena lies beyond the boundaries of linguistics properly so-called. In practice however 'sociolinguistics' continues to be used as a cover name for a variety of different topics ranging from linguistic demography at one end to the sociology of knowledge at the other; and the question arises to what extent these very disparate areas of enquiry have anything in common. Is there any sort of integrated picture of the relation of language to other social phenomena, a general framework expressing the social meaning of language, to which these studies relate and through which they relate to each other? It is the intention of the present paper to explore this question. It may be said in advance that, while nothing so definite as a conclusion will be reached, the terminal direction will be towards integration — towards eliminating boundaries rather than imposing them, and towards a unifying conception of language as a form of social semiotic.

Sociolinguistics sometimes appears to be a search for answers which have no questions. Let us therefore enumerate at this point some of the questions that do seem to need answering.

1. How do people decode the highly condensed utterances of everyday speech, and how do they use the social system for doing so?

2. How do people reveal the ideational and interpersonal environment within which what they are saying is to be interpreted? In other words, how do they construct the social contexts in which meaning takes place?

This paper incorporates material from the author's 'Sociological aspects of semantic change', in Luigi Heilmann & Carlo Mastrelli (eds.), *Proceedings of the Eleventh International Congress of Linguists*, Bologna: Il Mulino.

3. How do people relate the social context to the linguistic system? In other words, how do they deploy their meaning potential in actual semantic exchanges?

4. How and why do people of different social class or other subcultural groups develop different dialectal varieties and different orientations towards meaning?

5. How far are children of different social groups exposed to different verbal patterns of primary socialization, and how does this determine their reactions to secondary socialization especially in school?

6. How and why do children learn the functional-semantic system of the adult language?

7. How do children, through the ordinary everyday linguistic interaction of family and peer group, come to learn the basic patterns of the culture: the social structure, the systems of knowledge and of values, and the diverse elements of the social semiotic?

(1) Some areas of sociolinguistic research

A list of topics that come under the heading of sociolinguistics might include the following:

1. Macrosociology of language; linguistic demography
2. Diglossia; multilingualism and multidialectalism
3. Language planning; development and standardization
4. Pidginization and creolization
5. Social dialectology; description of non-standard varieties
6. Educational sociolinguistics
7. Ethnography of speaking; speech situations
8. Register; verbal repertoire and code switching
9. Social factors in phonological and grammatical change
10. Language and socialization; language in the transmission of culture
11. Sociolinguistic approach to language development in children
12. Functional theories of the linguistic system
13. Linguistic relativity
14. Microsociology of knowledge (ethnomethodological linguistics)
15. Theory of text

We shall try to take up some of the questions that have been being investigated under these headings, in order to suggest where they link up with each other and to see how far they already form part of a general pattern.

1.1 Linguistic interaction. Somehow the participants in speech encounters interpret one another's symbolic behaviour; they assign each other roles and statuses, accept and act on instructions and explanations, and in general exchange meanings which derive from every kind of social context. They do this first and foremost by attending to *text*, which is language in a context of situation — language in the environment of other semiotic structures and processes. But we have very little conception of *how* they do it. Cicourel (1969) suggests that participants operate with four interpretative

principles, 'reciprocity of perspectives', 'normal forms', 'the "etcetera" principle' and 'descriptive vocabulary as indexical expressions'. In other words, each individual assumes that others (i) see things in the same way as he does, (ii) agree on what to leave out, (iii) fill in what has been left out and (iv) use language in the same way to refer to past experience. These principles act as 'instructions for the speaker-hearer for assigning infinitely possible meanings to unfolding social scenes'.

Similar ideas are embodied in ethnomethodological linguistic studies such as those of Sacks and Schegloff, for example Schegloff's account of how people refer to location. Schegloff (1971) shows that the speaker derives from the context of situation the relevant criteria for deciding which of a number of possible strategies for identifying places and persons is the 'right' one in the particular circumstances; and he concludes that 'interactants are context-sensitive'. This is similar to the way in which a speaker selects the appropriate information focus, distributing the text into meanings that he is treating as recoverable to the hearer ('given') and meanings he is treating as non-recoverable ('new') (Halliday, 1967*b*). It is important to stress that a speaker also has the option of being 'wrong' — of deliberately organizing the meaning in a way that runs counter to the context of situation, with marked rhetorical effect.

The difficulty of integrating linguistic interaction studies with other areas of sociolinguistic research lies mainly in the lack of an explicit formulation of the relationship between the text, which is the *process* of interaction, and the linguistic *system*. How are the participants exploiting their semantic potential? And how does this potential relate systematically to features of the context of situation? We need answers to these questions if we are to make generalizations about how semiotic acts are encoded in language and linguistic meanings interpreted as semiotic acts.

1.2 The ecology of speech. From a sociolinguistic standpoint a text is meaningful not so much because the hearer does not know what the speaker is going to say, as in a mathematical model of communication, as because he does know. He has abundant evidence, both from his knowledge of the general (including statistical) properties of the linguistic system and from his sensibility to the particular cultural, situational and verbal context; and this enables him to make informed guesses about the meanings that are coming his way.

The speaker's selection of options in the production of text is regulated by the 'theory and system of speaking' in the culture (Hymes, 1967). Hymes postulates that the member has access to a set of sociolinguistic principles or 'rules of speaking', so that he knows 'when to speak and when to remain silent, which code to use, when, where and to whom, etc.'; in Grimshaw's (1971) interpretation, he makes 'generalizations about relationships among components' of the speech situation. What are the components of the speech situation? Hymes' own formulation may be summarized as: the form and content of the message, the setting, the participants, the intent and the effect of the communication, the key, the medium, the genre, and the norms of interaction. An example of an earlier formulation is provided by Firth (1950): the participants — their statuses and roles, the relevant features of the setting, the verbal and non-verbal action, and the effective result.

The difficulty of relating these notions to other sociolinguistic concepts lies in the fact that we do not know what kind of theoretical validity to ascribe to lists such as these. What are we to understand by 'situation', and what is its relation to the text? Both Firth and Hymes include the text itself among the 'features' or 'components' of the situation: Firth's 'verbal action of the participants', Hymes' 'form and content of the message'. But we shall need to conceive of the 'situation' in more abstract terms, and of situational features as *determinants* of the text, enabling us to predict what the speaker is going to say in the same way that the hearer does (cf. Hymes, 1971). Otherwise it is impossible to relate the text systematically to its environment (or Hymes' ethnography of speaking to Sacks' theory of linguistic interaction).

1.3 Functional theories of language. These have been mainly of four kinds: anthropological, e.g. Malinowski (1923, 1935); psychological, e.g. Bühler (1934); ethnological, e.g. Morris (1967); or educational, e.g. Britton (1970). All these have in common the property of being extrinsic in orientation: they are not concerned with language as object but with language in the explanation of other phenomena. Hence they are meant to be interpreted as generalizations about language use rather than as explanations of the nature of the linguistic system (cf. Greenberg, 1963, Chapter 7).

All these theories incorporate in one form or another the basic distinction between two primary semiotic roles that language serves: an ideational role, that of *being about* something, and an interpersonal role, that of *doing* something. Essentially the same distinction is expressed in the pairs of terms 'narrative/active'; 'representational (or 'informative')/expressive and conative'; 'cognitive/social'; 'semantic/stylistic'. Hymes uses the terms 'referential/social' (also 'socio-expressive' and 'stylistic'); he interprets the distinction as one between types of meaning expressed by different linguistic resources: 'Languages have conventional units, structures and relations ... that are what I shall call "stylistic" (serving social meaning) as well as referential' (1969). Linguists of the Prague school recognized a third component, which they called 'functional sentence perspective'; this is the text-forming or textual role that language serves, and it is a role that is purely intrinsic to language (Daneš (ed.), in press). So there is a general conception of language as serving two major functions; to which a third has to be added, of a somewhat different kind because intrinsic to language itself, if the functions are to be related systematically to linguistic structure.

The difficulty with the concept of functions of language, and its relation to a general sociolinguistics, is that the functions as usually conceived are neither concrete enough nor abstract enough. On the one hand it is difficult to relate them to the text, to what people actually say, since people nearly always seem to be using language in more than one 'function' at once; and on the other hand it is difficult to relate them to the linguistic system, because there are not, in fact, any recognizable linguistic entities — words, or grammatical constituents — that can be identified as serving just this or just that function, as expressing one type of meaning and not others. The problem arises through a false equation of 'function' with 'use'. It is necessary to separate these two concepts in the sociolinguistic context, and

also to suggest how they may come to be separated developmentally in the course of the learning of the mother tongue (Halliday, 1974).

1.4 Variety, variation and variability. It has always been recognized that dialectal variety in language reflected the social as well as the geographical provenance of the speaker, but it was Labov's highly original studies of urban speech patterns which effectively extended the scope of dialectology from the regional to the social dimension (1966).

Unlike rural dialects, which could be and traditionally have been treated as systems of invariant forms, urban dialects display patterns of inherent variation. A city-dweller, at least in our society, typically switches among a range of different forms of a given variable, in general without being aware that he is doing so. A number of factors are involved in this switching, such as monitoring (adjustment to meet the social conditions of the speech situation) and marking (adjustment for the purpose of signalling special social roles) (Labov, 1970); but all of them in one way or another derive from the nature of social dialect as a manifestation of the social structure and particularly of its hierarchical nature.

A closely related phenomenon is that of code switching, studied in detail by Gumperz (1971, Part II), in which speakers in multilingual contexts regularly switch between different languages, often within a sentence or even smaller unit. This, likewise, reflects the relative status, and also the functional specialization, of each language in the society in question.

Labov has demonstrated very convincingly that variation is inherent in the linguistic system. His work shows that, over and above the kind of variation that consists in socially motivated departure from an essentially stable norm, we have to recognize variability: that is, the system itself embodies variables, to some of which social values then tend to accrue.

The difficulty with variation and variability as *socio*linguistic concepts (cf. Bickerton, 1971) is to know what is the nature of the social meanings that are being realized through these patterns. How do the variants differ in meaning, and what is the *meaning* of choosing one rather than another? In other words, what is the semantics of the social structure that they are being made to express? It is not too difficult to relate variation in language to a general concept of subjective social stratification which has been derived from the study of this variation in the first place. The problem is to go further than this, by relating the linguistic phenomena to, and integrating them into, an independently established social theory with its own interpretation of social structure and social change.

1.5 Language, social structure and education. It is not difficult to demonstrate that, where there is a high rate of educational failure in urban areas, this failure tends to be associated with social class; roughly, the lower the family on the social scale (whether this is assessed intuitively or by means of some standardized measures) the greater the child's chances of failure.

Investigators concluded from this that there was a linguistic element in the situation; in some sense, language was to blame. If lower working class children show significantly greater discrepancy between measures of verbal and non-verbal intelligence, a discrepancy which tends to increase with age, then their language is holding them back; and this must mean their dialect, since it is the dialect that is distinctive. So educational failure is explained as language failure, with language interpreted as social dialect (Baratz & Shuy, 1969; Shuy, n.d.; Williams, 1970).

The language failure theory has taken two alternative forms, usually known as 'deficit' and 'difference'. The deficit version lays the blame on language as a system. According to this version, the language of the children concerned is deficient in some respect — it has not enough words, or not enough structures. The difference version lays the blame on language as an institution. It holds that the language of the children who fail is not deficient, but that the fact that it is different (from the standard language) acts just as much to their disadvantage because the standard language is required by the educational process, or else simply by social prejudice on the part of teachers and others. Either way, the child suffers.

Neither version of the theory is satisfactory, though for different reasons: the deficit version because it is not true, the difference version because, although it is true, it does not explain. There is no convincing evidence that children who fail in school have fewer words and structures at their command than those who succeed; and the notion of a defective dialect is in any case self-contradictory. But if children are failing because they speak a dialect that is different, they can certainly learn a second one, as children do in many cultures the world over. (This is not to deny, of course, that the attitudes towards their own speech forms are harmful and unjust. In fact, non-standard-speaking children often do learn standard speech forms, though not always for scholastic purposes.) This argument does not in any way destroy the sociolinguistic concept of a social dialect, as a language variety that is related to the social structure, one which expresses and symbolizes social hierarchy. But it removes it from the centre of the picture of the educational crisis.

1.6 Language and cultural transmission. What then are the significant social class differences in language, if any, and what are the social processes that give rise to them? Bernstein was the first to suggest an interpretation in terms of the transmission of the culture from one generation to the next.

It is a commonplace that a child's primary socialization in family and peer group takes place largely through language. What Bernstein's work has shown is how the social structure comes to be represented and transmitted in the process. This takes place also through language. Bernstein postulated that linguistic interaction is regulated by sociolinguistic 'codes', or coding orientations, embodying two major variables: elaborated versus restricted, and person-oriented versus object-oriented. These determine the speech variants or types of discourse typically associated with particular situations. The meaning of 'orientation towards persons or objects' is clear. 'Elaboration or restriction' is more opaque; but an 'elaborated' variant is one which is more verbally explicit, which maintains social distance, demands individuated

responses and makes minimal assumptions about the hearer's intent; it thus tends towards less ambiguity in the situational reference and more ambiguity in the role relationships. The codes are general tendencies governing the range of meanings that speakers typically deploy.

The individual child's exposure to the codes is, Bernstein suggests, a function of the system of role relationships within the family; and in particular of the balance between the 'positional' system, in which role corresponds to ascribed status (the part played by the member is a function of position in the family), and the 'personal' system, in which role corresponds to achieved status (the part played by the member is a function of his qualities as an individual). All combinations are found in all social classes, but, in Britain at least, strongly positional role systems tend to be found mainly in lower working class families (where educational failure is high). It seems that a positional family structure tends to orient the members away from the elaborated, personal mode, in just those socializing contexts in which learning is associated with an adult authority figure, for example the context of parental control. But it is predominantly this mode that is demanded by the principles and processes of education as at present constituted. Hence the styles of meaning through which the culture is transmitted produce an incompatibility between lower working class social norms and the middle class ethos and the educational system that is based on it (Bernstein, 1971).

The difficulty here for a sociolinguistic theory is to understand how the codes are translated into linguistic interaction. By what mechanism do the differences in elaboration and orientation manifest themselves in the way people talk, to each other and to their children? Bernstein has made it clear from the start that these are not matters of social dialect, of varieties of lexicogrammatical and phonological semantic systems, which as a general interpretation is implausible (and has also been rejected by Bernstein, at one point), or there must be some channel through which they intercede between the semantic system and the text.

1.7 Sociolinguistics and language development. The learning of the mother tongue can be interpreted from one point of view as a sociolinguistic process, and such an interpretation is one component of the general sociolinguistic universe. Aside from Bernstein's work, and that which is derivative from him, there are currently three main avenues of research which lead in the direction of a developmental sociolinguistics. One is through semantics, one is along the lines of 'sociolinguistic competence', and the third is via a functional approach to the linguistic system.

Recent interest in the semantics of language development arose as an extension of psycholinguistic studies in the learning of vocabulary and structure; hence it has tended to focus on word meanings, conceptual structures and logical relations (see Eve V. Clark, in press). The study of the acquisition of sociolinguistic competence is concerned with how the child learns the social uses of language, the 'rules of speaking' whereby his language meets the demands of the situation and the social structure (Susan M. Ervin-Tripp, 1972, 1973). In the functional approach, learning the mother tongue has been interpreted as learning the set of functions that language serves and developing a meaning potential in respect of each (Halliday, 1974).

Each of these three approaches presents certain difficulties, including that of isolation from the other two. Work in the field of child semantics has focused on the acquisition of concepts as an aspect of cognitive development; but it has not sought to relate meaning to social context, or to interpret language as the realization of social meanings and the semantic system as the linguistic encoding of the social system. The concept of sociolinguistic or communicative competence derives from the acceptance of a sharp distinction between a highly idealized 'competence' and a correspondingly belittled 'performance', a distinction which is at best irrelevant, and at worst obstructive, in a sociolinguistic perspective; this tends to isolate the system from its use, and hence to obscure the fact that the system develops through interaction, as a meaning potential that is always related to social contexts. The problem for the functional approach is that of showing how meanings evolve in a functional context, and how a postulated initial set of developmental functions of language come to be incorporated into the linguistic system (cf. 1.3 above). Finally the problem for all three approaches is to give some indication of how, and why, a child develops a linguistic system that has just the properties that human language has, and of how, and why, the human species developed such a system in the first place.

(2) Elements of a sociosemiotic theory of language

In this section we shall refer to certain general concepts, inherent in these and related sociolinguistic studies, which form essential ingredients in any social-interactional theory of language. These are the text, the situation, the text variety or register, the code (in Bernstein's sense), the linguistic system (including the semantic system), and the social structure.

2.1 Text. Let us begin with the concept of *text*, the instances of linguistic interaction in which people actually engage: whatever is said, or written, in an operational context, as distinct from a citational context like that of words listed in a dictionary.

For some purposes it suffices to conceive of a text as a kind of 'super-sentence', a linguistic unit that is in principle greater in size than a sentence but of the same kind. It has long been clear, however, that discourse has its own structure that is not constituted out of sentences in combination (sometimes referred to as a 'macro' structure; cf. van Dijk, 1972); and in a sociolinguistic perspective it is more useful to think of text as *encoded* in sentences, not as composed of them. (Hence what Cicourel refers to as omissions by the speaker are not so much omissions as encodings, which the hearer can decode because he shares the principles of realization that provide the key to the code.) In other words, a text is a semantic unit; it is the basic unit of the semantic process. It may be instantiated in various ways, as speech act, speech event, topic unit, exchange, episode, narrative and so on.

At the same time, text represents choice. A text is 'what is meant', selected from the total set of options that constitute what can be meant. In other words, text can be defined as actualized meaning potential.

The meaning potential, which is the paradigmatic range of semantic choice that is present in the system, and which the members of a culture have access to in their language, can be characterized in two ways, corresponding to

Malinowski's distinction between the 'context of situation' and the 'context of culture' (1923, 1935). Interpreted in the context of culture, it is the entire semantic system of the language. This is a fiction, something we cannot hope to describe. Interpreted in the context of situation, it is the particular semantic system, or set of sub-systems, which is associated with a particular type of situation or social context. This too is a fiction; but it is something that may be more easily describable (cf. 2.5 below). In sociolinguistic terms the meaning potential can be represented as the range of options that is characteristic of a specific situation type.

2.2 Situation. The situation is the environment in which the text comes to life. This is a well-established concept in linguistics, going back at least to Wegener (1885). It played a key part in Malinowski's ethnography of language, under the name of 'context of situation'; Malinowski's notions were further developed and made explicit by Firth, who maintained that the context of situation was not to be interpreted in concrete terms as a sort of audio-visual record of the surrounding 'props' but was, rather, an abstract representation of the environment in terms of certain general categories having relevance to the text. The context of situation may be totally remote from what is going on round about during the act of speaking or of writing. Firth's characterization was referred to in 1.2 above.

It will be necessary to represent the situation in still more abstract terms if it is to have a place in a general sociolinguistic theory; and to conceive of it not as situation but as situation *type*, in the sense of what Bernstein refers to as a 'social context'. This is, essentially, a semiotic structure. It is a constellation of meanings deriving from the semiotic system that constitutes the culture.

If it is true that a hearer, given the right information, can make sensible guesses about what the speaker is going to mean — and this seems a necessary assumption, seeing that communication does take place — then this 'right information' is what we mean by the social context. It consists of those general properties of the situation which collectively function as the determinants of text, in that they specify the semantic configurations that the speaker will typically fashion in contexts of the given type.

However, such information relates not only 'downward' to the text but also 'upward', to the linguistic system and to the social system. The 'situation' is a theoretical sociolinguistic construct; it is for this reason that we interpret a particular situation type, or social context, as a semiotic structure. The semiotic structure of a situation type can be represented as a complex of three dimensions: the ongoing social activity, the role relationships involved, and the symbolic or rhetorical channel. We shall refer to these respectively as *field*, *tenor* and *mode* (following Halliday, McIntosh & Strevens, 1964, as modified by Spencer and Gregory, 1964; and cf. Gregory 1967). The field, corresponding roughly to Hymes' 'setting' and 'ends', is the field of social action in which the text is embedded; it includes the subject-matter, as one special manifestation. The tenor, which corresponds in general to Hymes' 'participants' and 'key', is the set of role relationships among the relevant participants; it includes levels of formality as one particular instance. The mode, roughly Hymes' 'instrumentalities' and 'genre', is the channel or wavelength selected, which is essentially the function that is assigned to language

in the total structure of the situation; it includes the medium (spoken or written), which is explained as a functional variable.

Field, tenor and mode are not kinds of language use, nor are they simply components of the speech setting. They are a conceptual framework for representing the social context as the semiotic environment in which people exchange meanings. Given an adequate specification of the semiotic properties of the context in terms of field, tenor and mode we should be able to make sensible predictions about the semantic properties of texts associated with it. To do this, however, requires an intermediary level — some concept of text variety, or register.

2.3 Register. The term *register* was first used in this sense, that of text variety, by Reid (1956); the concept was taken up and developed by Jean Ure (Ure & Ellis, 1972), and interpreted within Hill's (1958) 'institutional linguistic' framework by Halliday, McIntosh & Strevens (1964). The register is the semantic variety of which a text may be regarded as an instance.

Like other related concepts, such as 'speech variant' and '(sociolinguistic) code' (Ferguson, 1971, Chapters 1 & 2; Gumperz, 1971, Part I), register was originally conceived of in lexicogrammatical terms. Halliday et al. drew a primary distinction between two types of language variety: dialect, which they defined as variety according to the user, and register, which they defined as variety according to the use. The dialect is what a person speaks, determined by who he is; the register is what a person is speaking, determined by what he is doing at the time. This general distinction can be accepted, but, instead of characterizing a register largely by its lexicogrammatical properties, we shall suggest, as with text, a more abstract definition in semantic terms.

A register can be defined as the configuration of semantic resources that the member of a culture typically associates with a situation type. It is the meaning potential that is accessible in a given social context. Both the situation and the register associated with it can be described to varying degrees of specificity; but the existence of registers is a fact of everyday experience — speakers have no difficulty in recognizing the semantic options and combinations of options that are 'at risk' under particular environmental conditions. Since these options are realized in the form of grammar and vocabulary, the register is recognizable as a particular selection of words and structures. But it is defined in terms of meanings; it is not an aggregate of conventional forms of expression superposed on some underlying content by 'social factors' of one kind or another. It is the selection of meanings that constitutes the variety to which a text belongs.

2.4 Code. *Code* is used here in Bernstein's sense; it is the principle of semiotic organization governing the choice of meanings by a speaker and their interpretation by a hearer. The code controls the semantic styles of the culture.

Codes are not varieties of language, as dialects and registers are. The codes are so to speak 'above' the linguistic system; they are types of social semiotic, or symbolic orders of meaning generated by the social system (cf.

Hasan, 1973). The code is actualized in language through the register, since it determines the semantic orientation of speakers in particular social contexts; Bernstein's own use of 'variant' (as in 'elaborated variant') refers to those characteristics of a register which derive from the form of the code. When the semantic systems of the language are activated by the situational determinants of text — the field, tenor and mode — this process is regulated by the codes.

Hence the codes transmit, or control the transmission of, the underlying patterns of a culture or sub-culture, acting through the socializing agencies of family, peer group and school. As a child comes to attend to and interpret meanings, in the context of situation and in the context of culture, at the same time he takes over the code. The culture is transmitted to him with the code acting as a filter, defining and making accessible the semiotic principles of his own sub-culture, so that as he learns the culture he also learns the grid, or sub-cultural angle on the social system. The child's linguistic experience reveals the culture to him through the code, and so transmits the code as part of the culture.

2.5 The linguistic system. Within the linguistic system, it is the *semantic system* that is of primary concern in a sociolinguistic context. Let us assume a tri-stratal model of language, with a semantic, a lexicogrammatical and a phonological stratum; this is the basic pattern underlying the (often superficially more complex) interpretations of language in the work of Troubetzkoy, Hjelmslev, Firth, Jakobson, Martinet, Pottier, Pike, Lamb, Lakoff and McCawley (among many others). We can then adopt the general conception of the organization of each stratum, and of the realization between strata, that is embodied in Lamb's stratification theory (Lamb, 1971, 1974).

The semantic system is Lamb's 'semological stratum'; it is conceived of here, however, in functional rather than in cognitive terms. The conceptual framework was already referred to in 1.3 above, with the terms *ideational*, *interpersonal*, and *textual*. These are to be interpreted not as functions in the sense of uses of language, but as functional components of the semantic system — 'metafunctions' as we have called them elsewhere (Halliday, 1974). (Since in respect both of the stratal and of the functional organization of the linguistic system we are adopting a ternary interpretation rather than a binary one, we should perhaps explicitly disavow any particular adherence to the magic number three. In fact the functional interpretation could just as readily be stated in terms of four components, since the ideational comprises two distinct subparts, the experiential and the logical; but the distinction happens not to be very relevant here.)

What are these functional components of the semantic system? They are the modes of meaning that are present in every use of language in every social context. A text is a product of all three; it is a polyphonic composition in which different semantic melodies are interwoven, to be realized as integrated lexicogrammatical structures. Each functional component contributes a band of structure to the whole.

The ideational function represents the speaker's meaning potential as an observer. It is the content function of language, language as about something. This is the component through which the language encodes the cultural experience,

and the speaker encodes his own individual experience as a member of the culture. It expresses the phenomena of the environment: the things — creatures, objects, actions, events, qualities, states and relations — of the world and of our own consciousness, including the phenomenon of language itself; and also the 'metaphenomena', the things that are already encoded as facts and as reports. All these are part of the ideational meaning of language.

The interpersonal component represents the speaker's meaning potential as an intruder. It is the participatory function of language, language as doing something. This is the component through which the speaker intrudes himself into the context of situation, both expressing his own attitudes and judgments and seeking to influence the attitudes and behaviour of others. It expresses the role relationships associated with the situation, including those that are defined by language itself, relationships of questioner-respondent, informer-doubter and the like. These constitute the interpersonal meaning of language.

The textual component represents the speaker's text-forming potential; it is that which makes language relevant. This is the component which provides the texture; that which makes the difference between language that is suspended *in vacuo* and language that is operational in a context of situation. It expresses the relation of the language to its environment, including both the verbal environment — which has been said or written before — and the non-verbal, situational environment. Hence the textual component has an enabling function with respect to the other two; it is only in combination with textual meanings that ideational and interpersonal meanings are actualized.

These components are reflected in the lexicogrammatical system in the form of discrete networks of options. In the clause (simple sentence), for example, the ideational function is represented by transitivity, the interpersonal by mood, and the textual by a set of systems that have been referred to collectively as 'theme'. Each of these three sets of options is characterized by strong internal but weak external constraints: for example, any choice made in transitivity has a significant effect on other choices within the transitivity systems, but has very little effect on choices within the mood or theme systems. Hence the functional organization of meaning in language is built in to the core of the linguistic system, as the most general organizing principle of the lexicogrammatical stratum.

2.6 Social structure. Of the numerous ways in which the social structure is implicated in a sociolinguistic theory, there are three which stand out. In the first place, it defines and gives significance to the various types of social context in which meanings are exchanged. The different social groups and communication networks that determine what we have called the 'tenor' — the status and role relationships in the situation — are obviously products of the social structure; but so also in a more general sense are the types of social activity that constitute the 'field'. Even the 'mode', the rhetorical channel with its associated strategies, though more immediately reflected in linguistic patterns, has its origin in the social structure; it is the social structure that generates the semiotic tensions and the rhetorical styles and genres that express them (Barthes, 1970).

Secondly, through its embodiment in the types of role relationship within the family, the social structure determines the various familial patterns of communication; it regulates the meanings and meaning styles that are associated with given social contexts, including those contexts that are critical in the processes of cultural transmission. In this way the social structure determines, through the intermediary of language, the forms taken by the socialization of the child. (See 1.6 above, and Bernstein, 1971, 1974).

Thirdly, and most problematically, the social structure enters in through the effects of social hierarchy, in the form of caste or class. This is obviously the background to social dialects, which are both a direct manifestation of social hierarchy and also a symbolic expression of it, maintaining and reinforcing it in a variety of ways: for example, the association of dialect with register — the fact that certain registers conventionally call for certain dialectal modes — expresses the relation between social classes and the division of labor. In a more pervasive fashion, the social structure is present in the forms of semiotic interaction, and becomes apparent through incongruities and disturbances in the semantic system. Linguistics seems now to have largely abandoned its fear of impurity and come to grips with what is called 'fuzziness' in language; but this has been a logical rather than a sociological concept, a departure from an ideal regularity rather than an organic property of sociosemiotic systems. The 'fuzziness' of language is in part an expression of the dynamics and the tensions of the social system. It is not only the text (what people mean) but also the semantic system (what they can mean) that embodies the ambiguity, antagonism, imperfection, inequality and change that characterize the social system and social structure. This is not often systematically explored in linguistics, though it is familiar enough to students of communication and of general semantics, and to the public at large. It could probably be fruitfully approached through an extension of Bernstein's theory of codes (cf. Mary Douglas, 1972). The social structure is not just an ornamental background to linguistic interaction, as it has tended to become in sociolinguistic discussions. It is an essential element in the evolution of semantic systems and semantic processes.

(3) A sociolinguistic view of semantics

In this section we shall consider three aspects of a sociological semantics: the semantics of situation types, the relation of the situation to the semantic system, and the sociosemantics of language development. The discussion will be illustrated from a sociolinguistic study of early language development.

3.1 The semantics of situation types.

A sociological semantics implies not so much a general description of the semantic system of a language but rather a set of context-specific semantic descriptions, each one characterizing the meaning potential that is typically associated with a given situation type (cf. 2.2 above; also Halliday, 1972). In other words, a semantic description is the description of a register.

This approach has been used to great effect by Turner in a number of studies carried out under Bernstein's direction in London (Turner, 1973). Turner's contexts in themselves are highly specific; he constructs semantic networks representing, for example, the options taken up by mothers in response

to particular questions about their child control strategies. At the same time they are highly general in their application, both because of the size of the sample investigated and, more especially, because of the sociological interpretation that is put upon the data, in terms of Bernstein's theories of cultural transmission and social change.

The sociolinguistic notion of a situation type, or social context, is variable in generality, and may be conceived of as covering a greater or smaller number of possible instances. So the sets of semantic options that constitute the meaning potential associated with a situation type may also be more or less general. What characterizes this potential is its truly 'sociolinguistic' nature. A semantics of this kind forms the interface between the social system and the linguistic system; its elements realize social meanings and are realized in linguistic forms. Each option in the semantic network, in other words, is interpreted in the semiotics of the situation and is also represented in the lexicogrammar of the text. (Note that this is not equivalent to saying that the entire semiotic structure of the situation is represented in the semantic options, and hence also in the text, which is certainly not true.)

Figure 1 shows an outline semantic network for a particular situation type, one that falls within the general context of child play; more specifically, it is that of a small child manipulating vehicular toys in interaction with an adult. The network specifies some of the principal options, together with their possible realizations. The options derive from the general functional components of the semantic system (2.5 above) and are readily interpretable in terms of the grammar of English; we have not attempted to represent the meaning potential of the adult in the situation, but only that of the child. The networks relate, in turn, to a general description of English, modified to take account of the child's stage of development.

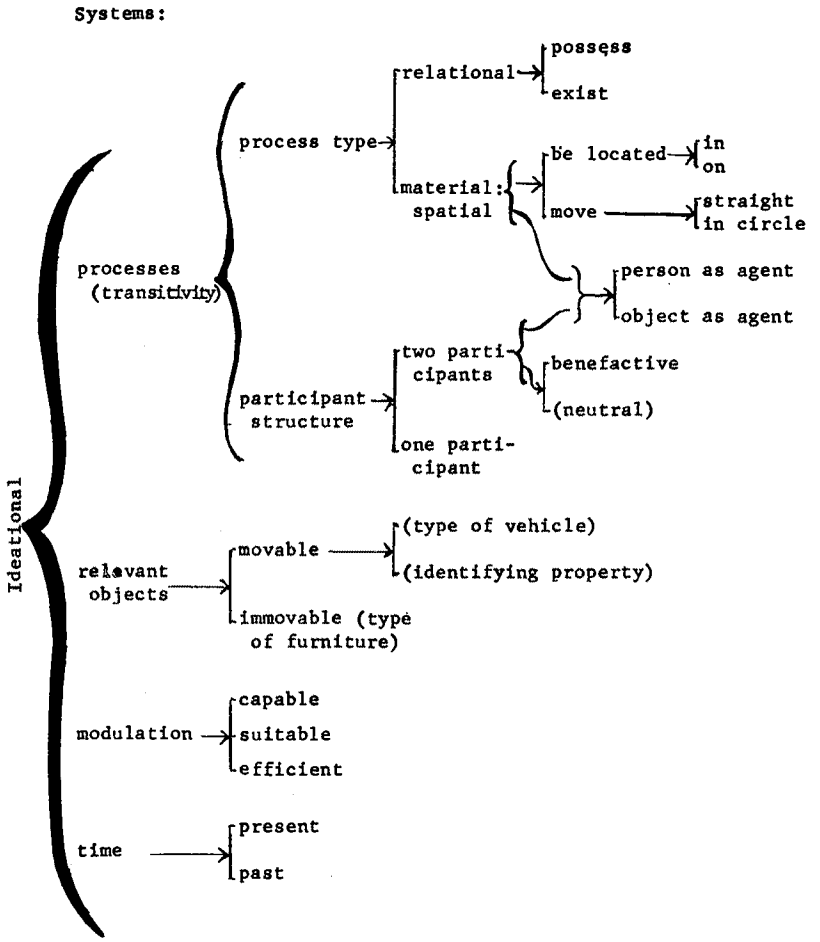
3.2 Structure of the situation, and its relation to the semantic system.

The semiotic structure of a situation type can be represented in terms of the three general concepts of field, tenor and mode (cf. 2.2 above). The 'child play' situation type that was specified by the semantic networks in Figure 1 might be characterized, by reference to these concepts, in something like the following manner:

Field. Child at play: manipulating movable objects (wheeled vehicles) with related fixtures, assisted by adult; concurrently associating (i) similar past events, (ii) similar absent objects; also evaluating objects in terms of each other and of processes.

Tenor. Small child and parent interacting: child determining course of action, (i) announcing own intentions, (ii) controlling actions of parent; concurrently sharing and seeking corroboration of own experience with parent.

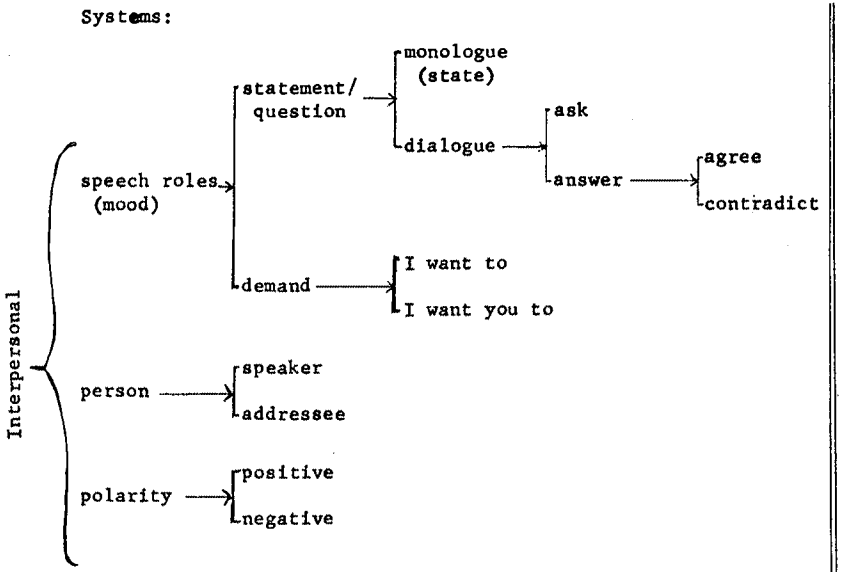
Mode. Spoken, alternately monologue and dialogue, task-oriented; pragmatic, (i) referring to processes and objects of situation, (ii) relating to and furthering child's own actions, (iii) demanding other objects; interposed with narrative and exploratory elements.



Realizations in text:

be located / 2 participants	put
,, / 1 participant	be (in, on)
be located : in	in
,, : on	on
move / 2 participants : person	send
,, ,, : object	carry
,, / 1 participant	go
move : straight	down
,, : in circle	round & round
possess / 2 participants	[give]
,, / 1 participant	have
exist / 2 participants	find
,, / 1 participant	be (there's)
benefactive ('for me')	for you
movable : type of vehicle	train, engine, lorry
,, : identifying property	blue, black
immovable	chair, floor, railway line
capable	will
suitable	(be) for
efficient	(go) well
past	(past tense)
present	(present tense)

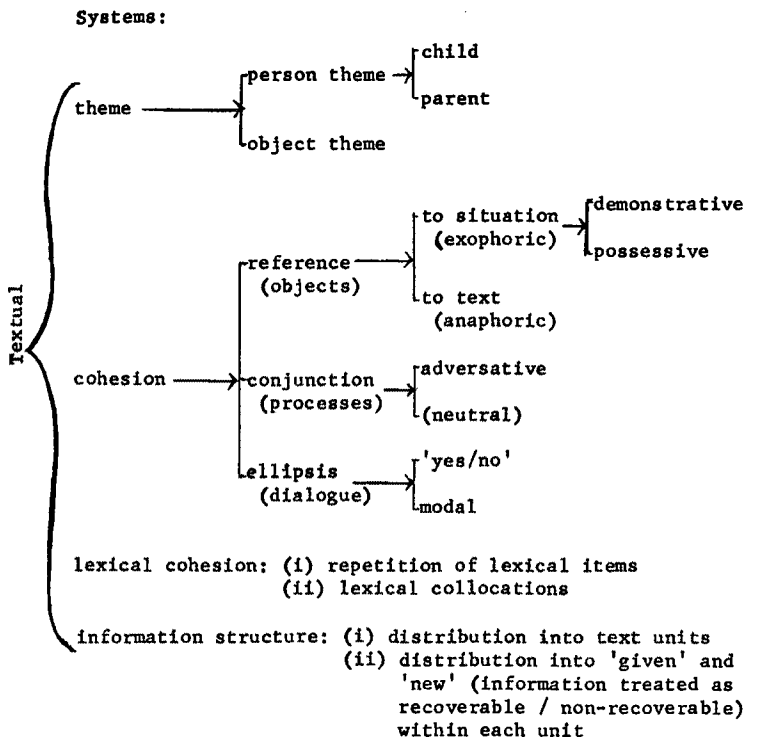
Figure 1 (a)



Realizations in text:

statement/question	(falling tone)
demand	(rising tone)
monologue / positive	(indicative)
,, / negative	(,, +) not
ask	I wonder (+ indicative)
answer [cf. textual component]	yes[/no] (+ indicative)
I want to / positive	(I/you) want; (subjectless non-finite, e.g. have that)
,, / negative	(I/you) don't want
I want you to	let me [sic]; (proper name); (I/you) want (proper name) to
speaker	I; you (sic)
addressee	you; (proper name, e.g. Daddy)

Figure 1 (b)



Realizations in text:	
person theme : child	I/you (initial); (subjectless non-finite)
,, : parent	(proper name initial)
object theme	(object name initial)
exophoric : demonstrative	this, that, the, here
,, : possessive	your ('my')
anaphoric	it, that, the
adversative	but; (fall-rise tone)
ellipsis : 'yes/no'	yes [no]
,, : modal	(modal element, e.g. it is, it will)
lexical : repetition of items	(e.g. train ... train)
lexical : collocations	(e.g. chair ... floor; train ... railway line)
information structure : text units	(organization in tone groups)
information structure : given - new	(location of tonic nucleus)

Figure 1 (c)

Figure 1. Semantic systems and their realizations, as represented in Nigel's speech (text in 3.2)

Below is a specimen of a text having these semiotic properties. It is taken from a study of the language development of one subject, Nigel, from nine months to three and a half years; the passage selected is from age 1;11. [Note: \ = falling tone; / = rising tone; V = fall-rise tone; tonic nucleus falls on syllables having tone marks; tone group boundaries within an utterance shown by ... . For analysis of intonation, cf. Halliday, 1967*a*.]

> *Nigel [small wooden train in hand, approaching track laid along a plank sloping from chair to floor]: Here the railway line ... but it not for the train to go on that. Father: Isn't it? Nigel: Yes tis. ... I wonder the train will carry the lorry [puts train on lorry (sic)]. Father: I wonder. Nigel: Oh yes it will. ... I don't want to send the train on this floor ... you want to send the train on the railway line [runs it up plank on to chair] ... but it doesn't go very well on the chair. ... [makes train go round in circles] The train all round and round ... it going all round and round ... [tries to reach other train] have that train ... have the blue train ('give it to me') [Father does so] ... send the blue train down the railway line ... [plank falls off chair] let me put the railway line on chair ('you put the railway line on the chair!') [Father does so] ... [looking at blue train] Daddy put sellotape on it ('previously') ... there a very fierce lion in the train ... Daddy go and see if the lion still there. ... Have your engine ('give me my engine'). Father: Which engine? The little black engine? Nigel: Yes ... Daddy go and find it for you ... Daddy go and find the black engine for you.*

Nigel's linguistic system at this stage is in a state of transition, as he approximates more and more closely to the adult language, and it is unstable at various points. He is well on the way to the adult system of mood, but has not quite got there — he has not quite grasped the principle that language can be used *as a substitute for* shared experience, to impart information not previously known to the hearer; and therefore he has not yet learnt the general meaning of 'yes/no question'. He has a system of person, but alternates between *I/me* and *you* as the expression of the first person 'I'. He has a transitivity system, but confuses the roles of Agent (Actor) and Medium (Goal) in a non-middle (two participant) process. It is worth pointing out perhaps that adult linguistic systems are themselves unstable at many points — a good example being transitivity in English, which is in a state of considerable flux; what the child is approximating to, therefore, is not something fixed and harmonious but something shifting, fluid and full of indeterminacies.

What does emerge from a consideration of Nigel's discourse is how, through the internal organization of the linguistic system, situational features determine text. If we describe the semiotic structure of the situation in terms of features of field, tenor and mode, and consider how these various features relate to the systems making up the semantic networks shown in Figure 1, we arrive at something like the picture presented in Figure 2.

There is thus a systematic correspondence between the semiotic structure of the situation type and the functional organization of the semantic system. Each of the main areas of meaning potential tends to be determined or activated by one particular aspect of the situation:

	Situational	Semantic
Field	manipulation of objects assistance of adult movable objects and fixtures movability of objects & their relation to fixtures recall of similar events evaluation	process type and participant structure benefactive type of relevant object type of location and movement past time modulation
Tenor	interaction with parent determination of course of action enunciation of intention control of action sharing of experience seeking corroboration of experience	person mood and polarity demand, 'I want to' demand, 'I want you to' statement/question, monologue statement/question, dialogue
Mode	dialogue reference to situation textual cohesion: objects textual cohesion: processes furthering child's actions orientation to task spoken mode	ellipsis (question-answer) exophoric reference anaphoric reference conjunction theme (in conjunction with transitivity and mood; typically, parent or child in demands, child in two-participant statements, object in one-participant statements) lexical collocation and repetition information structure

Figure 2

Determination of semantic features by elements of semiotic structure of situation (text in 3.2)

Semantic components		Situational Elements
Ideational	systems activated by features of	Field
Interpersonal	" " " " "	Tenor
Textual	" " " " "	Mode

In other words, the type of symbolic activity (field) tends to determine the range of meaning as content, language in the observer function (ideational); the role relationships (tenor) tend to determine the range of meaning as participation, language in the intruder function; and the rhetorical channel (mode) tends to determine the range of meaning as texture, language in its relevance to the environment. There are of course many indeterminate areas — though there is often some system even in the indeterminacy: for example, the child's evaluation of objects lies on the borderline of 'field' and 'tenor', and the system of 'modulation' likewise lies on the borderline of the ideational and interpersonal components of language (Halliday, 1969). But there is an overall pattern. This is not just a coincidence: presumably the semantic system evolved as symbolic interaction among people in social contexts, so we should expect the semiotic structure of these contexts to be embodied in its internal organization. By taking account of this we get an insight into the form of relationship among the three concepts of situation, text and semantic system. The semiotic features of the situation activate corresponding portions of the semantic system, in this way determining the register, the configuration of potential meanings that is typically associated with this situation type, and becomes actualized in the text that is engendered by it.

3.3 Sociosemantics of language development. A child learning his mother tongue is learning how to mean; he is building up a meaning potential in respect of a limited number of social functions (cf. 1.7 above). These functions constitute the semiotic environment of a very small child, and may be thought of as universals of human culture.

The meanings the child can express at this stage derive very directly from the social functions. For example, one of the functions served by the child's 'proto-language' is the regulatory function, that of controlling the behavior of other people; and in this function he is likely to develop meanings such as 'do that some more' (continue or repeat what you've just been doing), and 'don't do that'. How does he get from these to the complex and functionally remote meanings of the adult semantic system?

These language-engendering functions, or 'proto-contexts', are the origin at one and the same time both of the social context and of the semantic system. The child develops his ability to mean by a gradual process of generalization and abstraction, which in the case of Nigel appeared to go somewhat along the following lines. Out of the six functions of his proto-language (instrumental, regulatory, interactional, personal, heuristic and imaginative), he derived a simple but highly general distinction between language as a means of doing and language as a means of knowing — with the latter, at this stage, interpretable functionally as 'learning'. As he moved into the phase of transition into the adult system, at around 18 months, he assigned every utterance to one or other of these generalized functional categories, encoding the distinction by means of intonation: all 'learning' utterances were on a falling tone, and all 'doing' utterances on a rising tone. As forms of interaction, the latter

required a response (increasingly, as time went on, a *verbal* response) while the former did not.

From the moment when this semantic principle was adopted, however, it ceased to satisfy, since Nigel already needed a semiotic system which would enable him to do both these things at once — to use language in both the learning mode and the doing mode within a single utterance. Without this ability he could not engage in true dialogue; the system could not develop a dynamic for the adoption and assignment of semiotic roles in verbal interaction. At this point, two steps were required, or really one complex step, for effectively completing the transition to the adult system. One was a further abstraction of the basic functional opposition, such that it came to be incorporated into his semantic system, as the two components of 'ideational' and 'interpersonal'; in the most general terms, the former developed from the 'learning' function, the latter from the 'doing' function. The other step was the introduction of a lexicogrammar, or syntax, making it possible for these two modes of meaning to be expressed simultaneously in the form of integrated lexicogrammatical structures.

The term 'sociosemantics of language development' refers to this process, whereby the original social functions of the infant's proto-language are reinterpreted, first as 'macro-functions', and then as 'meta-functions', functional components in the organization of the semantic system. These components, as remarked earlier (2.5), are clearly seen in the adult language; the options show a high degree of mutual constraint within one component but a very low degree of constraint between components. At the same time, looked at from another point of view, what the child has done is finally to dissociate the concept of 'function' from that of 'use'; the functions evolve into components of the semantic system, and the uses into what we are calling social contexts or situation types. For a detailed treatment of this topic see Halliday (1974).

(4) Towards a general sociolinguistic theory.

In this final section we shall try to suggest how the main components of the sociolinguistic universe relate to one another, the assumption being that this network of relations is the cornerstone of a general sociolinguistic theory.

4.1 Meaning and text. The *text* is the linguistic form of social interaction. It is a continuous progression of meanings, combining both simultaneously and in succession. The meanings are the selections made by the speaker from the options that constitute the *meaning potential*; text is the actualization of this meaning potential, the process of semantic choice.

The selections in meaning derive from different functional origins, and are mapped on to one another in the course of their realization as lexicogrammatical structure. In our folk linguistic terminology, the 'meaning' is represented as 'wording' — which in turn is expressed as 'sound' ('pronouncing') or as 'spelling'. The folk linguistic, incidentally, shows our awareness of the tri-stratal nature of language.

4.2 Text and situation. A text is embedded in a context of *situation*. The context of situation of any text is an instance of a generalized social context or situation type. The situation type is not an inventory of ongoing sights and sounds but a semiotic structure; it is the ecological matrix that is constitutive of the text.

Certain types of situation have in their semiotic structure some element which makes them central to the processes of cultural transmission; these are Bernstein's 'critical socializing contexts'. Examples are those having a regulative component (where a parent is regulating the child's behaviour), or an instructional component (where the child is being explicitly taught).

4.3 Situation as semiotic structure. The semiotic structure of the situation is formed out of the three sociosemiotic variables of *field*, *tenor* and *mode*. These represent in systematic form the type of activity in which the text has significant function (field), the status and role relationships involved (tenor) and the symbolic mode and rhetorical channels that are adopted (mode). The field, tenor and mode act collectively as determinants of the text through their specification of the register (4.5 below); at the same time they are systematically associated with the linguistic system through the functional components of the semantics (4.4).

4.4 Situation and semantic system. The semiotic components of the situation (field, tenor and mode) are systematically related to the functional components of the semantics (ideational, interpersonal and textual): *field* to the *ideational* component, representing the 'content' function of language, the speaker as observer; *tenor* to the *interpersonal* component, representing the 'participation' function of language, the speaker as intruder; and *mode* to the *textual* component, representing the 'relevance' function of language, without which the other two do not become actualized. There is a tendency, in other words, for the field of social action to be encoded linguistically in the form of ideational meanings, the role relationships in the form of interpersonal meanings, and the symbolic mode in the form of textual meanings.

4.5 Situation, semantic system and register. The semiotic structure of a given situation type, its particular pattern of field, tenor and mode, can be thought of as resonating in the semantic system and so activating particular networks of semantic options, typically options from within the corresponding semantic components (4.4). This process specifies a range of meaning potential, or *register*: the semantic configuration that is typically associated with the situation type in question.

4.6 Register and code. The specification of the register by the social context is in turn controlled and modified by the *code*: the semiotic style, or 'sociolinguistic coding orientation' in Bernstein's term, that represents the particular sub-cultural angle on the social system. This angle of vision is a function of the social structure. It reflects, in our society, the pattern of social hierarchy, and the resulting tensions between an egalitarian ideology and a hierarchical reality. The code is transmitted initially through

the agency of family types and family role systems, and subsequently reinforced in the various peer groups of children, adolescents and adults.

4.7 Language and the social system. The foregoing synthesis presupposes an interpretation of the social system as a *social semiotic*: a system of meanings that constitutes the 'reality' of the culture. This is the higher-level system to which language is related: the semantic system of language is a realization of the social semiotic. There are many other forms of its symbolic realization besides language; but language is unique in having its own semantic stratum.

This takes us back to the 'meaning potential' of 4.1. The meaning potential of language, which is realized in the lexicogrammatical system, itself realizes meanings of a higher order; not only the semiotic of the particular social context, its organization as field, tenor and mode, but also that of the total set of social contexts that constitutes the social system. In this respect language is unique among the modes of expression of social meanings: it operates on both levels, having meaning both in general and in particular at the same time. This property arises out of the functional organization of the semantic system, whereby the meaning potential associated with a particular social context is derived from corresponding sets of generalized options in the semantic system.

4.8 Language and the child. A child begins by creating a proto-language of his own, a meaning potential in respect of each of the social functions that constitute his developmental semiotic. In the course of maturation and socialization he comes to take over the adult language. The text-in-situation by which he is surrounded is filtered through his own functional-semantic grid, so that he processes just what is interpreted in terms of his own social semiotic at any particular stage.

As a strategy for entering the adult system he generalizes from his initial set of functions an opposition between language as doing and language as learning. This is the developmental origin of the interpersonal and ideational components in the semantic system of the adult language. The concept of function is now abstracted from that of use, and has become the basic principle of the linguistic organization of meaning.

4.9 The child and the culture. As a child learns language, he also learns *through* language. He interprets text not only as being specifically relevant to the context of situation but also as being generally relevant to the context of culture. It is the linguistic system that enables him to do this; since the sets of semantic options which are characteristic of the situation (the register) derive from generalized functional components of the semantic system, they also at the same time realize the higher order meanings that constitute the culture, and so the child's focus moves easily between the microsemiotic and the macrosemiotic environment.

So when Nigel's mother said to him 'Leave that stick outside; stop teasing the cat; and go and wash your hands. It's time for tea.', he could not only understand the instructions but could also derive from them

information about the social system: about the boundaries dividing social space, and 'what goes where'; about the continuity between the human and the animal world; about the regularity of cultural events; and more besides. He does not, of course, learn all this from single instances, but from the countless sociosemiotic events of this kind that make up the life of social man. And as a corollary to this, he comes to rely heavily on the social system for the decoding of the meanings that are embodied in such day-to-day encounters.

In one sense a child's learning of his mother tongue is a process of progressively freeing himself from the constraints of the immediate context — or, better, of progressively redefining the context and the place of language within it — so that he is able to learn through language, and interpret an exchange of meanings in relation to the culture as a whole. Language is not the only form of the realization of social meanings, but it is the only form of it that has this complex property: to mean, linguistically, is at once both to reflect and to act — and to do both these things both in particular and in general at the same time. So it is first and foremost through language that the culture is transmitted to the child, in the course of everyday interaction in the key socializing agencies of family, peer group and school. This process, like other semiotic processes, is controlled and regulated by the code; and so, in the course of it, the child himself also takes over the coding orientation, the sub-cultural semiotic bias that is a feature of all social structures except those of a (possibly non-existent) homogeneous type, and certainly of all complex societies of a pluralistic and hierarchical kind.

4.10 Summary. Figure 3 is an attempt to summarize the discussion in diagrammatic form; the arrow is to be read as 'determines'. What follows is a rendering of it in prose.

Social interaction typically takes a linguistic form, which we call *text*. A text is the product of indefinitely many simultaneous and successive choices in meaning, and is realized as lexicogrammatical structure, or 'wording'. The environment of the text is the context of situation, which is an instance of a social context, or *situation type*. The situation type is a semiotic construct which is structured in terms of *field*, *tenor* and *mode*: the text-generating activity, the role relationships of the participants, and the rhetorical modes they are adopting. These situational variables are related respectively to the *ideational*, *interpersonal* and *textual* components of the *semantic system*: meaning as context (the observer function of language), meaning as participation (the intruder function) and meaning as texture (the relevance function). They are related in the sense that each of the situational features typically calls forth a network of options from the corresponding semantic component; in this way the semiotic properties of a particular situation type, its structure in terms of field, tenor and mode, determine the semantic configuration or *register* — the meaning potential that is characteristic of the situation type in question, and is realized as what is known as a 'speech variant'. This process is regulated by the *code*, the semiotic grid or principles of the organization of social meaning that represent the particular sub-cultural angle on the social system. The sub-cultural variation is in its turn a product of the *social structure*, typically the social hierarchy acting through the distribution of family types having different familial role systems. A child, coming into the picture, interprets text-in-situation in terms of his generalized functional categories

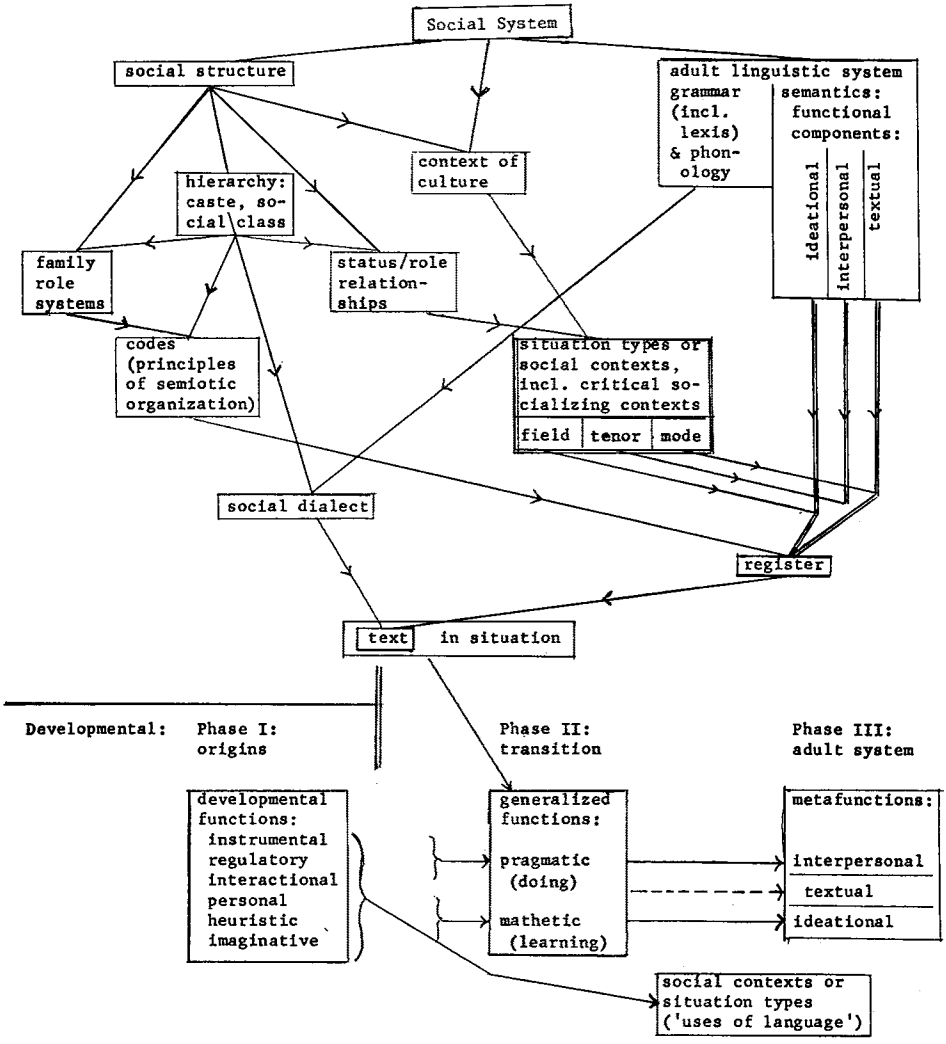


Figure 3

Scematic representation of language as social semiotic, and child's mode of access

of *learning (mathetic)* and *doing (pragmatic)*; from here by a further process of abstraction he constructs the functionally organized semantic system of the adult language. He has now gained access to the social semiotic; this is the context in which he himself will learn to mean, and in which all his subsequent meaning will take place.

The aim of this paper has been to interrelate the various components of the sociolinguistic universe, with special reference to the place of language within it. It is for this reason that we have adopted the mode of interpretation of the social system as a semiotic, and stressed the systematic aspects of it: the concept of system itself, and the concept of function within a system. It is all the more important, in this context, to avoid any suggestion of an idealized social functionalism, and to insist that the social system is not something static, regular and harmonious, nor are its elements held poised in some perfect pattern of functional relationships.

A 'sociosemiotic' perspective implies an interpretation of the shifts, the irregularities, the disharmonies and the tensions that characterize human interaction and social processes. It attempts to explain the semiotic of the social structure, in its aspects both of persistence and of change, including the semantics of social class, of the power system, of hierarchy and of social conflict. It attempts also to explain the linguistic processes whereby the members construct the social semiotic, whereby social reality is shaped, constrained and modified — processes which, far from tending towards an ideal construction, admit and even institutionalize myopia, prejudice and misunderstanding (Berger & Luckmann, 1967, Chapter 3).

The components of the sociolinguistic universe themselves provide the sources and conditions of disorder and of change. These may be seen in the text, in the situation, and in the semantic system, as well as in the dynamics of cultural transmission and social learning. All the lines of determination are *ipso facto* also lines of tension, not only through indeterminacy in the transmission but also through feedback. The meaning of the text, for example, is fed back into the situation, and becomes part of it, changing it in the process; it is also fed back, through the register, into the semantic system, which it likewise affects and modifies. The code, the form in which we conceptualize the injection of the social structure into the semantic process, is itself a two-way relation, embodying feedback from the semantic configurations of social interaction into the role relationships of the family and other social groups. The social learning processes of a child, whether those of learning the language or of learning the culture, are among the most permeable surfaces of the whole system, as one soon becomes aware in listening to the language of young children's peer groups — a type of semiotic context which has hardly begun to be seriously studied. In the light of the role of language in social processes, a sociolinguistic perspective does not readily accommodate strong boundaries. The 'sociolinguistic order' is neither an ideal order nor a reality that has no order at all; it is a human artefact having some of the properties of both.

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* This article was first written during my tenure of a fellowship at the Center for Advanced Study in the Behavioral Sciences, Stanford, California. I should like to express my gratitude to the Center for the opportunities which this afforded.

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THE NEED TO BE PRACTICAL

Robert J. Di Pietro
Georgetown University

(1) The relevance of contemporary linguistics. 'How does a linguist win the Nobel Prize?' It was a straight-forward question and the young man who asked it that day after my introductory lecture on the importance of being a linguist was honestly perplexed. The answer I ultimately gave him has been lost in the years that have passed. The question, however, lingers on, to re-surface occasionally -- whenever I begin to feel smug or complacent about my profession. Just how does a linguist win the Nobel Prize? I can think of many ways he can win one and not be a linguist. Scientists, philanthropists, peace-makers, writers of novels, and poets are all eligible for the coveted award presumably because their endeavours are in categories that somehow lead to the betterment of mankind. A likely candidate could be a linguist, but he would have to qualify for the Prize on other grounds. If, for example, a linguist were to find a way to stop wars for all times, he would certainly deserve a Nobel Prize, not to mention many other honors, perhaps even sainthood. In such a case, being a linguist would have no more relevance to the candidate than being a truck driver or a trapeze artist.

Perhaps we should resign ourselves to the opinion that the role of the linguist is a minor one in the arena of world affairs. After all, how enthusiastic should the public get over definitions of linguistics such as the following which was printed ten years ago in our professional journal Language (1962, vol. 38, p. 463):

> "A scientific linguist (also called a linguistic scientist, a linguistician, or most commonly in the profession, simply a linguist) is a specialist in linguistics, the systematic study of the structure and functioning of languages."

Where is the relevance to man's social and physical well-being to be found in this definition? Language goes on to mention the application of linguistic research to 'practical language problems' such as foreign language instruction and language planning for the emergent countries of the world. Yet, the precise ways in which linguistics can be applied often escapes even the most prominent spokesmen of our profession. Noam Chomsky, a man who has done much to bring linguistics before the public's eye has, himself, questioned the relevance of linguistics to language teaching (see Chomsky 1966). Even if mankind were finally to acknowledge the importance of linguistics, it is altogether possible that the linguist would not know how to make his own work relevant to the needs of mankind. Is T grammar more relevant than structural grammar because it is more elaborate or because it has received the gift of social variability by Labov and others? Tagmemics, which has not had the impact on the thinking of the profession that T grammar has had, is nonetheless used to great advantage by missionary linguists who would certainly defend the social relevance of what they are doing. If we were to draw some insidious comparisons with other fields, B. F. Skinner's behaviorist theories have attracted far more public interest than the attacks on those theories by linguists such as Chomsky. Why? Is psychology inherently more meaningful

to man than linguistics? Perhaps the path to greater human significance lies in redefining linguistics, as Chomsky (1968) does in his series of essays entitled Language and mind, as a branch of 'cognitive' psychology. I doubt, however, that much would be gained by relating language study to mental constructs -- especially since the notion of 'mind' is, itself, on rather shaky theoretical grounds.

The real problem appears to stem from how the linguist implicitly and perhaps unintentionally narrows his focus on language. Briefly put, the contemporary linguist is interested in grammar. He may associate it with mental constructs or with sociological factors. But whatever embellishment he gives his subject, the linguist of today remains essentially a grammarian. Despite the lip service paid to the definition of language as man's principal means of communicating with other men, the linguist goes on to concentrate his attention on the code of language, to wit, on its 'grammar'. By what alchemy do speculations about grammar lead to understanding the nature of how man communicates messages? Knowing the rules of grammar does not mean knowing the protocols by which those rules are employed in actual speech acts. If we can admit that descrepancies exist between 'grammar studies' and the study of man in communication and self-expression, we can also be convinced that any goal less than the latter is not practical for the linguist. My remarks from this point on will be made to diminish the central importance of grammar and to illustrate some of the ways in which linguistics can be redirected so as to make it more relevant to the study of man.

(2) How did transformational-generative theory really break away from structuralism? If we are to rearrange our commitments in linguistics, we must first understand in what ways the generative theories of today are significantly different from the structuralism they replaced. Chomsky's Syntactic structures (1957) made it popular among linguists to look upon language as a theoretically infinite number of sentences capable of being produced by a greatly restricted number of 'grammatical rules'. Pre-Chomskyan structuralists had the attitude that the linguist should commence his analysis with corpora or bodies of data from the native speaker. One of the effects of generative linguistics was to shift the analyst's attention from elicited utterances to what was potentially producible by a speaker. This shift in viewpoint had several ramifications. For one, it made it most fasionable to analyze one's own language, rather than attempt to work on others whose linguistic potentialities might be more inaccessible. Since the language of most generativists was English, the study of other languages diminished. An additional effect of the Chomskyan revolution was the great importance placed on judgements of grammaticality. Where the structuralist looked to his data for proof of existence, the generativist had to anticipate the production of sentences which may or may not be acceptable to speakers of the language other than himself. Since acceptability and grammaticality are not always synonomous with regard to grammar, the linguist can easily find himself in the anomalous situation of asserting that native speakers do not always produce grammatical sentences.

It has also been claimed that structural linguistics was taxonomic while transformational-generative linguistics was not. This distinction, I believe, is a trivial one. Taxonomies can be organized in any number of ways. A list

of generative rules shared or not shared by two languages would constitute just as much of a taxonomy as a set of descriptions in structural terms.

What is most important, in my opinion, is that generative linguistics as proselytized by Chomsky and his followers is not so different from structuralism as one might think. Both orientations share the same focus on language. Both the structuralist and the generativist view language as a code. Neither one has been overly concerned with the 'functioning' aspect of language mentioned in the definition of linguistics quoted from Language, above. The 'function' of language has certainly not been understood to mean the use of the code in communication. When, for example, Fillmore and others have spoken about the function of case relationships, their frame of reference has been syntactical. Fillmore (1968) provides a number of transformational operations to be performed on sentences in order to determine the case 'function' of each noun. Verbs are then classified in terms of 'case frames', or, in other words, in terms of how many different syntactical relationships they can maintain with nouns. Chafe (1970) comes somewhat closer to interrelating code structure with use of code in so far as he extrapolates his grammatical discussions from the kinds of situations in which a given language form might be employed. He writes (Chafe 1970, p. 66):

> "...it is obviously much easier to explain language use on the basis of the semanticist model. When we use language we start with something that we have to say -- with meanings. If we are going to communicate these meanings, they must be structured in a way that conforms to the semantic structure of our language."

Because Chafe's interest remains in explaining the code of language, the theory that emerges is not one of communication. In a chapter entitled 'New and old information' (Chafe 1970), only one kind of communication is considered, namely, the transfer of information. Then, rather than be concerned with the ways in which the participants in a speech act interact, Chafe concentrates on how this information affects the code. Communication presumably involves at least two individuals, but Chafe's concern remains with the structure of what is being said by one individual.

I do not mean to be unduly critical of either Chafe or Fillmore. Both scholars have contributed significantly to the linguistic study of language code. In fact, my criticisms might not be relevant to their latest positions (see, for example, Fillmore 1972). In any event, Fillmore's The case for case and Chafe's Meaning and the structure of language stand as monuments to language interpreted as code. My intention has been to point the way to a linguistics which would concern itself directly with man in communication with other men and in self expression through language. In some ways, language use is a contemporary way of talking about rhetoric and we should remember that the distinction between grammar and rhetoric goes all the way back to classical times. But, as Hymes (1971) points out, the ancient Greeks put a greater emphasis on rhetoric than they did on grammar in the education of their youth. Linguists in turning their attention to matters of language use will find much more that needs to be said that can be said in terms of grammar. Linguistic competence, if we wish to preserve the notion, must encorporate both the speaker's knowledge of a language's rules and his

knowledge of how to use those rules in specific circumstances. As far as I know, contemporary studies of linguistic competence have focussed on rules and have not taken up the matter of rule protocols.

(3) Some serious problems with the application of Cartesian philosophy to linguistics. Chomsky found that the philosophy of Descartes was hospitable to his kind of language study. While Descartes himself did not engage in language analysis, a tradition of grammar was motivated by his influence at Port Royal in France. It was a kind of grammar based on what emanates from the thought of individual persons. It was to be found by introspection -- much the way that one finds rationalistic mathematics. It had no interest in language as communication or even as the expression of human emotion or feeling. Rationalism has left a bequest of non-communicative grammars based on idealized single speakers -- individuals without personality who stand for everybody and nobody at the same time.

Unlike the mathematics that Descartes founded, rationalistic grammars were not recoverable according to an explicit set of operations. There was as much room for variation in grammar description -- beyond what became traditional and therefore accepted without question -- as there were rationalistic grammarians working on grammar. By the same token, the generative grammars written today are neither proven nor disproven; they are only shown to be more or less 'powerful' than alternative grammars. What is 'intuitively correct' to one generativist may not be so to another. If we look for 'psychological' validation, our search very often becomes one of finding data to fit an orientation already established for the sake of experimentation. In validating grammars, we only get answers to the questions we ask. We have no way of knowing about other questions which might be worth asking. Our only hope to break this lockstep is to change the theoretical orientation which nutured the questions we are asking.

Rationalistic grammars require that certain precepts be established as universal in nature. A universal base or framework is necessary to achieve some sort of truth-value for what is claimed. There are, of course, many ways to cast universals in language but, basically, they are either 'substantive' or 'formal' in nature. The substantive universals relate to categories which are supposedly immutable regardless of the language in question. Formal universals have to do with the interrelation of grammatical phenomena. That is, given X, then Y must also be present. In either case, it is difficult to distinguish between what is universal in language code and what is universal in the theory of language code. Thus, if verbs are considered to be universal, then so are nouns -- which constitute the class of forms to which verbs are opposed.

One way of validating universals is to ground them on empirical observation. In the case of rationalistic grammars, empiricism often takes the form of psychological proofs. Since thought and language are extremely difficult to distinguish, language universals based on mental constructs are as questionable as the mental constructs themselves. In any event, Chomsky, for one, is not overly concerned with this matter. He writes (Chomsky 1968, p. 12):

> "It seems to me that the most hopeful approach today is to describe the phenomena of language and of mental activity as accurately as possible, to try to develop an abstract theoretical apparatus that will as far as possible account for these phenomena and reveal the principles of their organization and functioning, without attempting, for the present, to relate the postulated mental structures and processes to any physiological mechanisms or to interpret mental function in terms of 'physical causes'."

Just how our 'abstract theoretical apparatus' will reveal the organization and functioning of language without some empirical grounding is not explained. Chomsky seems to be encouraging us to go on hypothesizing about language in the hopes that, some day, some of our hypotheses will touch upon the reality of language.

At the heart of the problem of indeterminacy in grammar is the mistaken opinion of many that grammar-construction is 'scientific'. Grammars, whether they be written by linguists in an effort to capture the code structure of a language or whether they are 'acquired' or 'learned' by an individual, are more artifact than natural organism. In other words, grammars are the artifacts produced by man's logic when faced with the need of processing and sorting language data. There is no reason to assume that there is one theoretically correct, immutable grammar of English or of any other language any more than there is one, perfect automobile engine. If the linguist is to persist in his interest in codes and wishes to operate more as a scientist than as an engineer, he must give up trying to write the most 'reasoned' grammar and concern himself, instead, with how the grammatical artifacts constructed by various people reflect man's human instinct to create symbols and communicate via them. Such a re-orientation would, in my opinion, be more realistic than one which places grammar writing directly in the center of the linguist's universe. It would show the world that linguists are aware of the restricted nature of grammatical artifacts. The writing of grammars or, if you prefer, the description of the grammatical competenees of individuals, is properly an endeavour of 'applied' linguistics. It is more engineering than science. But, as in engineering, it should be an application which identifies the natural principles upon which it is based (see discussion in Di Pietro 1971, p. 56-58). Unfortunately, the grammar of a language is, for many rational linguists, equal to the theory of that language. How can one hope to be practical if one does not know the difference between practice and theory?

Grammars, even precise ones, are limited in many significant ways. For one, they are unable to distinguish between what is 'literal' and what is 'metaphorical' in language. Generative grammars are constructed to produce only literal sentences. Attempts (see, for example, Thomas 1969) to make generative grammars sensitive to metaphor are basically operations which realign semantic features or block certain syntactical rules from applying to marked strings of words. Yet, before such operations can be programmed, the grammarian must decide that certain feature alignments and certain sentences are non-metaphorical. It is these sentences which motivate his description from the start. Rather than be peripheral to language code, metaphor may be the very motivating force in its creation. Chomsky (1965)

hints at the extent of creativity in language, but his own theory, based as it is on fixed rules of grammar, is unable to account for all of it (see Di Pietro 1970). If we continue to accept the centrality of grammar in linguistic endeavours and, in grammar, the centrality of syntax, the explanation of metaphor is reduced to showing, ad hoc, how formal rules can be changed or 'violated'. Grammar grows from the intellect and not from the senses. Metaphors are the linguistic expression of man's perceptions, of his awareness of being. It does not seem to be by chance that metaphorical extensions are usually made from the non-abstract, concrete to the abstract, rather than the other way around. Man's access to his world is through his ability to touch, smell, taste, hear and see it. The linguist can ill afford to ignore the impact these five senses must have on human language.

The cutting off of the senses and the reliance on mental introspection which characterize Cartesian linguistics have seriously weakened its explanatory power while paradoxically appearing to broaden the base of language study. In the past decade, we have seen linguists expand their interests to the study of child language acquisition and to building models of language in societal structure. In both endeavors, however, the procedure has often been to start with the same introspected grammar and adapt it to new settings. As a result, the linguistically naive pediatrician still knows more than the average linguist about the stages in child language development and the sociologist interested in communication (e.g., Goffman 1972) is bolstered in his belief that he has gone beyond anything the linguist has said about language. The net result for the linguist is that he has much more to learn from his colleagues in language-related fields than he has to teach them.

(4) Some new ways to expand language study. It should be clear by now that there are many concerns beyond grammar writing which should occupy the linguist. A theory of code, no matter how elegant it is, does not automatically lead to theories of communication or expression via language. Man is not only a builder of codes, he is a user of them. Both their structure and their use stem from his unique status as a human. There is an intimate interrelationship of language with all of man's being. The cerebral dominance which characterizes man's brain is associated with his development of hand preference. Both cerebral dominance and hand preference are connected with the development of speech. Children with delayed hand preference are often those who have retarded development in speech. It is also apparent that forcing a child to switch hand preference may also lead him to stutter.

The pronouns of a language are more than grammatical forms which somehow replace nouns. They mark the many relationships between speaker, hearer and referrent which grow from the social status of the parties involved, the setting of the speech act, and even the visual orientation of the speaker. Freedman, Cannady and Robinson (1971), in a study of the language of handicapped children, point out that blind children are much slower than sighted children in acquiring use of the first person pronoun ('I'). Apparently, some kind of visual recognition of the limitations of self is important in making the significance of this pronoun felt by the speaker. Fillmore (1972) illustrates this point with the example of a telephone conversation. The initiator of such a conversation does not begin with 'Hello, I am Mr. Smith'. Since the hearer is out of sight of the speaker, he is more likely to say: 'Hello, this is Mr. Smith'.

The growth of language in man is reflected not only by the maturation of his logico-deductive powers but also by the channeling of affect and emotion. Lili Peller (1964) perceives a very close connection between cognition and emotion. She writes (p. 61):

> "the young child's exuberant narcissism drives him to use proto-symbols long before his intellectual power enables him to generalize widely in the cognitive field. Perhaps it is this early narcissistic symbolization which makes possible the later symbolization in cognitive matters."

That the transference of information is only one of several uses of language in communication is shown by Charlotte Kursh (1971) in an excellent paper entitled 'The benefits of poor communication'. There are many situations in which humans use language to avoid confrontations which might be unpleasant. The conscious lie can have immediate benefits -- especially to a speaker who finds himself the physical inferior to the hearer and in danger of bodily harm. The so-called 'white lie' (e.g., saying Mrs. Smith is not in when she is) can be used very creatively, with little 'social cost'. Kursh also discusses the 'pseudocommunicational event' in which 'neither effective speaking nor effective listening is rewarded' (Kursh 1971, p. 202). In such situations, both parties are engaged in shouting down the other. Kursh's feeling that successful administrators need to learn how to mix truth and falsehood in artful ways is borne out by the recent accusations of lying against national leaders brought out in the so-called 'Andersen papers'. Certainly the linguist should study all kinds of communicational events in which language is used.

Paul Byers (1971), in making a plea for a 'new view of man' which breaks down the old boundaries between arts and science, finds that communication among humans is organized according to a natural rhythm or beat. By observing the interactions of a group of Eskimos involved both actively and passively in the same situational context, he found that each individual's actions were initiated on a shared beat with intervals of .4 of a second. There were no random onsets, even though the individuals involved may have appeared to be performing unrelated tasks. He hypothesized that the symbolizing capacity of human language is related to this natural rhythm and to the time interval (.2 of a second) within which man's neurological system and muscular apparatus are committed to completing a pre-programmed state before beginning another. Victor Yngve (1972ms) shares with Byers the view that social interaction and communication are based on 'human communalities'. Such communalities include man's limited memory span and the structure of his transmitting and receiving organs. In assuring that linguistics become more 'human', Yngve would reject any definition of the linguist's tasks as being limited to the study of signs, speech, and messages. According to Yngve, the linguist is a scientist whose principal interests lie in how people communicate. All of the linguist's specific concerns must lead to that end.

(5) <u>How to win that prize</u>. In finding ways to convince the world of our importance, we linguists might very well find ourselves struggling to rise to our own expectations of that importance. Grammar has not inspired mankind to find more equitable modes of life or even to understand

communication more clearly. Since man derives more pleasure from poetry, there has never been an aesthetics of grammar. Yet, the linguist cannot abandon the writing of grammars any more than he can deny all interest in the structure of linguistic code. What is needed is a reassessment of grammar as an exercise in programming rather than as a scientific explanation or description. 'Deep' structure, if we wish to preserve the notion, should be recognized as a grammatical mapping of the functional relations among language forms, rather than any direct portrayal of human thought. By the same token, semantics is only that part of meaning which is statable in terms of the grammatical segments we call 'features'. The most natural use of language is in self-expression and in conveying messages of all kinds to other humans. Speech acts, themselves, are either dyadic (involving a separate speaker and hearer) or monadic (not intended for the transfer of messages).

One might surmise that studies of language code are of greater importance with regard to dyadic speech acts than they are to the use of language in self-expression. Yet, language codes can be studied from two points of view: (1) how they become conventionalized, i.e., agreed upon by different members of a community of speakers, and (2) how the encoding constituents, themselves, relate to the speaker. The former point involves the socialization of language code and its change. The latter yields a picture of man as an individual who creates, uses and is affected by his own linguistic tools.

At the same time, the linguist cannot afford to ignore the messages being communicated via the code. Whereas the nature of the code may make it possible for man to form an infinitely large number of unique sentences, man's sociocultural constraints may lead him to restrict the number of different messages he needs to communicate. No matter how he may say it, man must greet other men, take leave of them, conduct customary daily affairs, and enter into legal transactions. And he must perform all of these functions of language according to the restrictions of his society. Whereas it might be possible to say 'It's a lovely day' in Arabic, such a statement would not fit the social requirements of opening a conversation with an Arab camel driver in the Sahara. Surely, the specific information transferred on encountering a camel driver would not be identical to that of the English-speaking school teacher greeting her class. The messages might be the same but the actual utterances would not share the same 'deep' structure, grammatically speaking.

While the study of language codes and the messages conveyed by them could occupy much of the linguist's time, there are other aspects of man the communicator which the linguist might find to be even more significant. Recent research by a small but growing number of scholars from different fields has centered on the structure of conversations. Earlier, I mentioned the observations of Byers (1971), regarding the rhythmic nature of communication. There are also overt language forms which participants in a conversation employ for various purposes of communication. Goffman (1972) speaks of 'connectives' -- devices used by the passive participant to encourage the active one to continue. If the participants are intimiate friends and the subject under discussion is sensational news, the connectives take the form of "Wow!" or "Gee!". Such connectives would hardly be appropriate in the doctor's office. A patient recounting symptoms of an illness would expect entirely different signs of encouragement to come from the doctor -- such as

"Yes" (with rising intonation) or "Go on", or even a nod of the head. Expressions of solidarity between a pastor and his congregation at a revival meeting would certainly not be in keeping within the format of a lecture on a technical subject at a university.

Coherence in either a conversation or a monolog does not depend solely on grammatical constraints, as some might think. Molly Bloom's long soliloquy in James Joyce's Ulysses is an excellent example of textual integrity which is preserved while grammaticality is strained to the breaking point.

In view of the many ways in which language use is connected to its users and their setting, genuinely ambiguous utterances are rare. In fact, the problem of structurally ambiguous sentences is as much of a pseudoproblem for linguists as is the question of grammaticality. As L. & H. Gleitman (1970) point out, people do not normally stop to judge the grammaticality of what has been said to them. Either they understand or they do not. If they do not, they ask for a paraphrase. If the listener realizes that an utterance might have several interpretations, he asks for a clarification. Otherwise the listener acts as if the utterance is perfectly clear to him -- despite the grammarian's paper-and-pencil proof that it should have been ambiguous. In a recent newspaper account of an airliner highjacking, it was reported that the highjacker gave the stewardess a note which read simply "Grenade pin pulled, pistol". No ambiguity was perceived.

Perhaps the greatest impact of man's creativity in language is felt when we examine the speech of individuals. The view of man as a maker of codes, as a member of a social class or as a participant in a conversation which has an inevitable end circumvents that creativity. Man conforms as a member of society or as a participant in a social act, but he creates new ideas or artifacts as a free-thinking individual. Studying the expression of individualized creativity through language (both as affect and as cognition) brings the linguist as close to man's humanity as he can possibly come. Who could deny a Nobel Prize to one who achieves such ennobling goals?

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II

Rumbles Within The Ruling Paradigm

ON THE PASSIVE IN ENGLISH

Dwight Bolinger
Harvard University

In the brave antediluvian days of generative grammar, the passive transformation was the rock that leveled the defenses of structuralism, and for a long time after continued to be the refuge for all who needed a quick and handy proof of the validity of transformations. Yet one by one, restrictions have hedged it about. Anna Hatcher reminded the 1957 Texas Conference that reflexives do not readily passivize: *Her eyes were blinked by her. Many idioms were found to resist being tampered with: *The story (the news) was broken by the reporter. It gradually became clear that there were important differences of meaning, not of truth value but of such things as topicalization and depersonalization: She was mad because she had been hit by a car (?by someone). Functional sentence perspective turned out to be a factor: The horse, which was brought to America by the Spaniards (?which the Spaniards brought to America), quickly became the Indian's personal companion (cf. Svartvik 1966: 166). Case grammar revealed the problems of non-agentive subjects: *Water is composed by hydrogen and oxygen (Yi-chin Fu 1966: 52). Finally, individual verbs or individual senses of verbs were seen to behave in unpredictable ways. Some could be used only in the active: John will fix him!; *He'll be fixed by John! Others could be used only in the passive: She is so misunderstood (emotionally) by her husband; *Her husband misunderstands her (emotionally);[1] as Winter (1965: 488-9) points out, few would feel that How old was your mother when you were born? and How old was your mother when she bore you? are equivalent sentences. Most allowed deletion of the agent, but some did not: We were met by our friends, We were met; We were joined by our friends, *We were joined. Traditional grammar was well aware of many of these difficulties (Svartvik 1966: 1-5), and Robin Lakoff (1971) has pointed out additional ones, especially the failure to account for get passives.

Despite the criticisms and disagreements, it has generally been held that passivization is a grammatical process and that barring some intrusions of idiom, the exceptions could be explained grammatically. Thus without inquiring into the motives of the passive and the reflexive as they relate to each other, it seemed to be sufficient to state the restriction as a grammatical rule, roughly to the effect that "reflexivization blocks passivization". Yet even here a passive may be more normal than an active:

> Being distrusted by a close friend is like being distrusted by oneself.
> ?Having a close friend distrust one is like having oneself distrust one.

The restriction with which this paper is concerned is that of the supposed intrusion of idiom -- or more exactly, lexicalized combination -- in the passive of prepositional verbs. I define the latter to include simple verb plus preposition (to account for) and also ditransitives (to make fun of):

> The discrepancies were easily accounted for.
> Nobody likes to be made fun of.

The restriction in its crudest form can be illustrated by examples like:

Leaves dangle from the limbs.
*The limbs are dangled from by leaves.

The shed stands between the house and the barn.
*The house and the barn are stood between by the shed.

They fired the gun at John.
*John was fired the gun at.

We entered the figures in the ledger.
*The ledger was entered the figures in.

A casual glance at the verbs that any corpus is apt to turn up -- Svartvik 1966: 165 lists account for, call for, care for, call upon, deal with, depend upon, dwell upon, fire on, go into, look at, preside over, provide for, talk of, think of -- suggests an obvious explanation for their use in passive constructions: they perform the same function as single verbs. In fact, it is frequently possible to find a one-word equivalent: account for = explain, call for = require or summon, care for = tend, call upon = invoke, etc. Svartvik defines them as "verbs and prepositions that have sufficient cohesion to be able to operate together", and he explains the difference between two such examples as:

The subject was gone into thoroughly.
*The room was gone into at once.

in terms of "some concept like concrete/abstract subject or literal/metaphorical verb...." Labov 1972: 777 is more lenient, remarking the "considerable latitude in the analysis of various...verb + preposition combinations" and offering This wall was stood beside as an acceptable example. But he then narrows the bounds for ditransitives much as Svartvik does for the nounless type: "the only vb + N + prep combinations which can fit into this analysis are idioms which have been lexicalized, like make fun of, take umbrage at." If a sentence such as *This bed was eaten potato chips in is felt to approach acceptability, it is because we assume for the moment "that eat potato chips in is such a vb." Hudson 1967: 244 fn., citing the examples:

Thermometers were made use of.
The weather was taken advantage of.

expresses much the same idea: "clauses whose direct object and verb are particularly closely connected lexically, and which must also contain a preposition-phrase whose preposition is selected by the verb and direct-object together". Mincoff 1958: 185 refers to "verbal phrases which are equal in function to a simple verb."[2] In short, the direct connection is not to the meanings of the expressions but to their lexical status. Ideally it should be possible to list the particular lexical entries, and Svartvik implies the hope that it may some day be done: "the limited and as yet not clearly defined range of prepositional verbs" (165).

In place of the lexical theory I propose a semantic one in which two factors mainly determine whether a passive will be acceptable or not: first,

the semantic relationship between the prepositional verb and the patient, which needs to be reexamined for simple verbs as well as prepositional ones; second, the interpretability of the sentence. I claim that while a running frequency count might well show that the great majority of prepositional verbs are lexicalized, lexicalization is only contributory. Passive prepositional verbs are a completely open set.

I begin with ditransitives, the more complex case, first to bury outright the notion that free combinations are ungrammatical, and second to dispose of the problem of interpretability before moving on to the meaning of transitivity.

It would be hard to claim that three such prepositional verbs as to fight a battle over, to stick pins in, and to whisper dirty innuendoes about are lexicalized in any normal sense of the term. Yet all three yield acceptable passives:

> That city has been fought many a battle over.
> He has been burned, stuck pins in, beheaded -- all in effigy, of course.
> To be whispered such dirty innuendoes about was enough to break any girl's heart.

What makes a ditransitive acceptable on the same terms as an ordinary prepositional verb is interpretability. We reject a sentence like *I don't like to be blamed mistakes on in spite of the acceptability of the corresponding active Don't blame those mistakes on me and of similar passives like I don't like to be thrown mud at because of the high degree of expectation that blame will have a personal and not an abstract object: the hearer gets as far as blamed and is sure that I don't like to be blamed means what it appears to mean -- and then mistakes on forces him to revise. Even though theoretically the same cumulative ambiguity afflicts That argument was built a case against (first, That argument was built; second, That argument was built a case), the distracting interpretations are less insistent. It would be difficult to enumerate all the factors that help to neutralize such distractions, and more still to classify them neatly, but the following will give a rough idea of what they are like; all have in common the fact that they help the listener to quickly identify the prepositional verb as a unit:

1. Contextual parallelism. In the last example, stuck pins in is wedged between burned and beheaded. (Even so, ?thrust swords through would be doubtful, so that we have to look for more than one factor operating at the same time). Other examples:

> The defendants -- the ones arrested and brought charges against yesterday -- are all expected to plead innocent. (?The defendants were brought charges against.)
> The books that were separated and stuck markers in are on the middle shelf.
> This tool has never been used for its main purpose -- in fact, it's never been done anything with at all.

2. Analogy, especially with indirect object ditransitives. The pattern represented by:

Mary was written a letter.
John was done a favor.
I don't like to be played a dirty trick like that.
That's a problem that needs to be found a remedy.

is of course commonplace, and may often be extended by the corresponding preposition; these examples are arranged from least to most likely to be thus extended:

Mary was written a letter to.
John was done a favor for.
I don't like to be played tricks on.
That's a problem that needs to be found a remedy for.

To the extent that other ditransitives resemble these, they become proportionately acceptable. It is as if we still had a residual sense of an underlying dative of interest:

Cheapskates like you are never stolen money from.
People like them are seldom written stories about.
I don't want to be told lies about.
Multiple sclerosis was one of the causes that were provided help for.
These people have never been done anything for (against, to, about).
She was shown concern for. (Cf. She was shown concern.)

In the following pair, make as a causative eases the way for make 'produce':

What's needed is something that can be made (into) breakfast food.
What's needed is something that can be made breakfast food out of.

Make also illustrates the effect of an interfering analogy. The supportive analogies can be seen in both of the following:

He likes to be máde things (for).
He doesn't like to be made a fóol (of).

yet they are in potential conflict through the accent patterns. If instead of the empty word things a word such as toys is used, thereby forcing the accent to move to the same position as with fóol, the result is confusing:

*He likes to be made tóys for.

3. The use of empty verbs, which tend to suggest that a following noun is to be expected as their completion. Examples with make have already been cited. Thus to make dents in = to dent, to put supports under = to support, to hold a threat over = to threaten, etc. These are not precisely lexicalized compounds, but might be regarded as highly lexicalizable ones; that is, the verbs are in the nature of compounding elements:

I don't like to be held a threat over.
If these bridges are put supports under, they will withstand any amount of flooding.
Throw that apple away. It's been taken at least three bites on.
That product can't be made a profit from.
What's he miffed about? Was he taken a swipe at by somebody?
These pans have already been made too many dents in.

The Walpole example[2] made a fighting ground of fits here. It is hard to decide whether take a swipe at should be regarded as an individually lexicalized item or not; if it is, then a kind of lexicalized receptable has to be admitted, in which not only swipe but also poke, stab, whack, shot, and probably some other nouns referring to aggressive acts may be contained. Take aim at is less fully lexicalized in this respect than draw a bead on.

A comparison of verbs of this group with those of the preceding (indirect object ditransitive) one would reveal an extensive overlapping. Possibly one reason why the indirect object verbs lend such strong support is that like make, take, etc. they "need" a noun complement.

4. Predictability of the verb + noun combination. This is the broad category that embraces lexical combinations:

John was held a grudge against.
The troops were opened fire on.
His objections were made too much of.
I don't like to be made a fool (dunce, idiot, monkey, scapegoat, patsy) of.
That whole platoon was simply made mincemeat of.
John is always being found fault with.
I want these things to be made use of.[3]

But lexicalization is a matter of degree, and predictability may also relate to the act itself: if it is something that is frequently done or spoken about, then its verbalization is accepted at face value. The example stick pins in contrasted with ?thrust swords through is an instance of such real-life frequency. Another example:

"How did he get bruised like that?" -- "He was thrown a club at or something, maybe just a stick of wood that was handy."

Examples of higher versus lower frequency combinations:

I don't like to be told lies about. (I don't like to be lied about.)
*I don't like to be spread lies about.

The solution has been raised serious doubts about.
?The solution has been raised serious questions about.

Hasn't that problem been come to any agreement about yet?
?Hasn't that problem been come to any understanding (compromise, conclusion) about yet?

That territory has been described, it has been made maps of, it has been trekked across -- all for what?
?That territory has been described, it has been drawn maps of...

In the last pair, the parallel construction is sufficient to raise to make maps of (= to map) to acceptability, but not to draw maps of. It is sometimes possible to set up a gradient of high to low frequency:

I don't like to be brought charges against.
?I don't like to be lodged charges against.
*I don't like to be filed charges against.

The line between a lexicalized and a high-frequency combination can be illustrated with make room for and find time (money, etc.) for -- the latter permits modification of the noun:

If it's worth having, then it can be made (*the) room for.
If it's a worthy undertaking, it can be found (the necessary) time (and money) for.

5. Repetition and presupposition. If the same verb + noun or something closely allied to it has already been mentioned, there will be less difficulty in interpreting them when they appear later in passive form. One of Hudson 1967's starred examples is *His ticket has been paid too much for (245). But there is no problem with this in a proper context:

"He paid too much for his coat." -- "Well, lots of things are paid too much for nowadays. You have to expect it."
"What's this pipe for?" -- "To blow bubbles with." -- Well, has it been?" -- "Been what?" -- "Blown any bubbles with yet?"
John is a hard person to get something on -- if he had ever been got anything on I'd have known about it.
"A voodoo doll is like Chinese medicine." -- "How's that?" -- "In one you're stuck pins in and in the other you're stuck (jabbed) needles in."

The last example can be compared with the unacceptable *In acupuncture you're stuck needles in. But even in these verbal expressions there is a certain predictability of the noun from the verb: to pay is normal with amounts, to blow with bubbles, to stick with pins, and among earlier examples, to fight with battles, to tell with lies, etc. Ask has a cognate object, question, which yields They were asked a lot of questions about. Compare the following gradient with reducing predictability:

He has been told lies about.
?He has been written lies about.
*He has been published lies about.

Just as important, probably, is the frequency of the verb itself: tell surpasses publish, bring surpasses lodge, and make surpasses any number of other verbs with which it may be synonymous:

> What's good about cellulose is that it can be made (*produced) sugar from.
> This pipe hasn't been made (?produced) any bubbles with yet.

Either make or produce is normal in the active: This pipe is to make (produce) bubbles with.

High-frequency verbs enter into high-frequency combinations with indefinites:

> That's a problem that should be done something about.
> You've brought up a point that I think can really be made something of.

I am not sure that any useful distinction can really be made between these verbs and the ones I referred to as "empty" above.

Returning to the optimizing effect of context, we find certain constructions more acceptable by reason of presupposing a prior context that is not necessarily explicitly given. In the following:

> *Were you aimed a gun at?
> How does it feel to be aimed a gun at?

the second contains a petitio principii: it cannot be asked unless the interlocutor has already had a gun aimed at him. I believe that topicalized infinitives sound better at least partly for this reason -- they imply that the matter has already been brought up:

> *This bed has been eaten potato chips in.
> To be eaten potato chips in isn't the best thing for a bed.

> ?The people were kept records on.
> To be kept records on is felt as a threat by most people.

> *This floor shouldn't be thrown a rug over.
> For a nice floor like this to be thrown a rug over would be a shame.

> *Houses as expensive as that shouldn't be put deposits on.
> For a house as expensive as that to be put a deposit on is foolish.

The same can be said of a denial of what has just been asserted:

> It wasn't put a deposit on, it was just made one or two bids on.

There are other ways in which a context can be propitiating. For example, the topic may already have been announced, and the over-all transitivity of the prepositional verb then becomes obvious:

> "What's gold good for?" -- "Well, it used to be coined money from (minted), but now it's too precious for that."
> Yeah, I know all about that stock. It was earned a nice profit from (made a lot of money with) in its time.

Similarly, the prepositional verb may be a kind of re-wording of something already said, by way of comment on it. Thus when said without preamble, even lexicalized combinations in sentences like:

> ?His arguments were taken umbrage at.
> ?His arguments were found fault with.

are awkward, but when properly introduced the same verbal group is normal:

> Everyone disapproved. Nothing that he said was accepted. Quite irrationally, his arguments were met with scorn. Yet afterward, in the calm light of reason, no one could understand why they were taken umbrage at (found fault with).

Other examples where it is implied that the topic has been introduced:

> ?John was made an enemy of.
> John is not a person to be made an enemy of.

> *John was brought charges against.
> John is not the kind of person who ought to be brought charges against.

An indefinite, referring to something previously introduced, may be acceptable where a noun is not:

> *I don't like to be blamed mistakes on.
> "What are you grumbling about? So you didn't do it, so what?" -- "I just don't like to be blámed things on."

> ?I don't want to be stuck needles in.
> "Are you afraid of acupuncture?" -- "No." -- "What's the matter, then?" -- "I just don't want to be stúck things (something) in."

While indefinites are no problem, the use of definite personal pronouns for the NPs of verbal groups I think is excluded:

> "How do you feel about lies?" -- "Í don't want to be told lies about!"
> "*Í don't want to be told them about!"
> "?Í don't want to be told any about!"

Nor is it a matter of definiteness, for other definite NPs are possible, or at least not quite so bad:

?If I were told those lies about I'd be pretty mad.

Other examples substituting pronouns for NPs in previous examples:

*Tricks are all right, but I don't like to played them on.
*Money is stolen every day, but cheapskates like you are never stolen it from. (OK stolen any from)

The problem is again interpretability. Even with indirect object ditransitives, personal pronouns as direct objects tend to be confusing. Sentences like:

Mary was written it (the letter).
We were given them (the gifts).
I don't need to be shown him.

are bothersome -- we prefer, for the last example, the explicit I don't need to have him shown to me.

Given all the possibilities of invention with verbal groups containing nouns -- far exceeding the lexicalized forms to which they were thought to be limited -- one understands the feeling that underlay Labov's remark about his potato chip example: "If this is not already acceptable in some dialect, we would enjoy meeting the man who had the imagination to say it" (1972: 777-8). The fact is that the only real restrictions are clarity and intent. The use of a prepositional verb has to be motivated. One motivation can be seen very clearly in the contrast between two of the examples cited above and their puristic counterparts:

The defendants -- those who were arrested and against whom charges were brought yesterday -- are all expected to plead innocent.
The books that were separated and in which markers were stuck are on the middle shelf.

Colloquial English avoids relatives with preposed prepositions. But there is also a semantic motive. The two arrangements, books...in which markers were stuck and books...stuck markers in, do not mean quite the same. The former suggests that the action to which it refers is new to the context. Compare:

The subjects that (as we know) were given examinations on....
The subjects on which examinations were given....

This probably reflects the difference between anaphoric that and interrogative which.[4]

A related problem of order arises in questions. Colloquially, the interrogative word has to come first. As a consequence, even though other motives are lacking (e.g., the parallelism in the "defendants" and "books" examples above), a question with an initial interrogative word and a final

preposition is an improvement on a similar declarative sentence and is no worse than its puristic counterpart:

*These bobbins have been put thread in.
?How many bobbins have been put thread in?
?In how many bobbins has thread been put?

*The top and middle shelves have been put books on.
?Which shelves have been put books on?
?On which shelves have books been put?

Mostly in this situation we avoid the construction altogether: How many bobbins have been threaded?, How many bobbins have had thread put in them?, How many bobbins have they put thread in?

To the extent that it is interpretable, a ditransitive prepositional verb has the same chances of success in the passive as one that lacks the extra noun.[5] One is as bad as the other if it violates the meaning of transitivity:

They did the work without John's help.
*John's help was done the work without.

The bullet ricocheted off the pavement.
*The pavement was ricocheted off by the bullet.

It is now time to inquire what that meaning is.

Without the extra variable to contend with in the shape of the noun, combinations of verb + preposition are comparatively easy to interpret. While the following are not acceptable:

*The chair was fallen from by John.
*Has the money been bought with?

we reject them not because they are unintelligible but because they are incongruous. The question is why.

The notion of idiomaticity has already been dealt with. While lexicalized combinations of verb and preposition are highly frequent in passives of this type:

It was never looked at.
She is often spoken of.
Hard drinks are best abstained from.
This has been waited for all too long.

there is no lack of acceptable improvised examples that can hardly be regarded as lexicalized:

The target was fired at.
This floor has obviously been walked on.
My car was run into last night.

To keep the issue clear, it is necessary to rule out the apparent passive. This is plain enough in cases of resultant condition like John is tired. The glass is broken, where English frequently offers forms other than regular past participles: The glass is full (filled); Our relations are over (finished). But in some cases it is not quite so clear that no event is involved. The test is whether the active counterpart has the same truth value:

Are you covered by insurance? (≠ Does insurance cover you?)
Are they governed by a dictator? (?≠ Does a dictator govern them?)
This one is painted by Rubens. (≠ Rubens paints this one.)

The approximate meanings of the first two examples are 'Is there insurance with reference to you?', 'Is there a dictator with reference to them?' Of course the counterpart of Does insurance cover (protect) you in a case like that? is a true passive.

Also it is necessary to exclude idioms which for no reason other than the fact that they are idioms do not passivize:

He kicked the bucket.
*The bucket was kicked by him.

They held his gaze.
*His gaze was held by them.[6]

-- and also idioms that make better passives than actives:

?I am happy that your presence graces my humble dwelling.
I am happy that my humble dwelling is graced by your presence.

Returning now to normal passives, the hypothesis I propose is this: the subject in a passive construction is conceived to be a true patient, i.e., to be genuinely affected by the action of the verb. If the grammatical object in the active construction is not conceived as a true patient, there will be no corresponding passive. The verbs may be simple or complex, and among the latter of course are prepositional verbs:

The chair was given to John. (The chair is viewed as affected.)
John was given the chair. (John is viewed as affected.)
The chair was sat on.
The chair was put varnish on.

The English passive neutralizes several cases and amalgamates them into a single archi-objective case:

The pen was broken. (Accusative)
The pen was given a cleaning. (Dative)
The pen was written with. (Instrumental)
The pen was put ink in. (Dative?)
The corpse was held a wake over. (?)
The capital has been visited by thousands of tourists. (Locative)

The only condition is that there must be a genuinely transitive relationship between verb and patient; this defines a true patient. The passive is marked for transitivity -- this confirms Anisfeld and Klenbort (1973: 118-20), who regard the passive as the marked mode.

The contrary case is a purely spatial or existential relationship. I refer to this as spatiality. It can be illustrated by simple verbs as easily as by prepositional ones. The examples:

1. The stranger approached me.
2. I was approached by the stranger.
3. The train approached me.
4. *I was approached by the train.

show, in the second pair, a purely spatial relationship in which me represents a terminus, not someone affected. The same meaning could be expressed by The train drew close to me, *I was drawn close to by the train, making the geometrical facts explicit. In 2 I am viewed as affected; the stranger perhaps was a panhandler. If 1 were intended in a purely spatial sense, the corresponding passive would be just as unacceptable as 4.

The semantic contrast is between true patients as defined and things or persons that are merely located with reference to others or to existence itself, i.e., presence on the scene, or absence. Since all prepositional verbs contain a preposition and a majority of prepositions have some kind of locative reference, whereas only a comparatively small number of simple verbs have underlying prepositions that may carry such a reference (approach = draw near to, reach = arrive at, contain = have in itself, etc.), we were forced to be aware of some kind of problem in the case of prepositional verbs but did not realize that the controlling factors are everywhere the same. If to the locative meaning of approach we add the meaning of transitivity, i.e., view the person or thing as affected, then the passive is normal. The same is true of prepositional verbs:

1. My brother has lived in Chicago.
2. *Chicago has been lived in by my brother.
3. Several famous personages have lived in the house.
4. The house has been lived in by several famous personages.
5. They arrived at the destination by five o'clock.
6. *The destination was arrived at by five o'clock.
7. They arrived at the conclusion by five o'clock.
8. The conclusion was arrived at by five o'clock.

In 4 the house has an aura by virtue of the people who have lived in it. In 8 there was no conclusion until the arrival -- arrive at creates the conclusion as surely as the verb in They consummated the conclusion creates it. But in 6 the destination is a preexisting point in space.

The conceptual difference between a true patient and a thing in space may show up through an association with mode:

1. My sister camped beside the lake.
2. *The lake was camped beside by my sister.

3. Nobody is to camp beside this lake!
4. This lake is not to be camped beside by anybody!
5. They got to Madrid in six hours.
6. *Madrid was got to in six hours.
7. You can't get to a place that far away in a short time.
8. A place that far away can't be got to in a short time.

In 4 'camping' is viewed as harming the lake. In 8 the place is viewed as an objective to be taken.

The difference may also show up with actions that are customary on the one hand, or planned on the other, around the object in question:

1. Jane ran up the stairs.
2. *The stairs were run up by Jane.
3. So many people have run up these stairs that the carpet is threadbare.
4. These stairs have been run up so much that the carpet is threadbare.
5. The dog walked under the bridge.
6. *The bridge was walked under by the dog.
7. Generations of lovers have walked under this bridge.
8. This bridge has been walked under by generations of lovers.
9. The racer went through Intersection No. 33.
10. *Intersection No. 33 was gone through.
11. None of the cars has gone through Intersection No. 33 yet.
12. Intersection No. 33 hasn't been gone through yet.
13. The boy walked through the room.
14. *The room was walked through by the boy.
15. These rooms weren't made to be walked through by every Tom, Dick, and Harry.

As in the example with house above, the patient in 8 has acquired an aura. As for 12, there is a planned route with objectives to be "taken" one by one.

The situation is the same with lexically simple verbs, except that underlying spatiality is less obvious. There is a tendency for true transitivity and superficial transitivity to coincide. In fact instances of spatiality may actually be traditional metaphors. When we say Water fills the glass (and reject *The glass is filled by water in favor of The glass is full of water or The glass is filled with water) we may be extending the transitive construction figuratively; but the figure does not extend to the passive. However this may be, the fact is that Water fills the glass is semantically no more than an expression of presence-on-the-scene, expressible with a preposition (There is water in the glass), which along with absence does not permit the passive:

1. The package contained a present.
2. *A present was contained by the package.
3. A superior force contained the enemy.
4. The enemy were contained by a superior force.[8]
5. The bottle holds ten quarts.

6. *Ten quarts are held by the bottle.
7. The poor lack money.
8. *Money is lacked by the poor.
9. John survived the accident.
10. *The accident was survived by John.[9]
11. This chapter includes other topics.
12. *Other topics are included by this chapter.
13. My friend lost a brother.
14. *A brother was lost by my friend.
15. My friend lost a dollar.
16. A dollar was lost by my friend.
17. They absented the premises.
18. *The premises were absented by them.
19. John occupied the chair.
20. ?The chair was occupied by John.
21. The troops occupied the city.
22. The city was occupied by the troops.
23. Snow covers the ground.
24. ?The ground is covered by snow.

In all these cases, presence-absence can be shown prepositionally: there is a present in the package, ten quarts are in the bottle, the poor are without money, John remains after the accident, there are other topics in the chapter, my friend is without his brother, they are away from the premises, John is in the chair, the snow is on the ground. The true transitives may carry prepositions too but there is more: restraint in 4, inattention in 16 (the paraphrases A brother was lost to my friend and *A dollar was lost to my friend are significant here), and force in 22. We can also see the gradience that is to be expected if metaphor is involved: 20 and 24 can be viewed as having true patients. The more vigorous the central meaning of the verb, the less likely it is that there is a purely metaphorical extension to 'presence-absence':

1. The scouts thronged (crowded) the square.
2. ?The square was thronged (crowded) by the scouts.
3. The river flooded the plain.
4. The plain was flooded by the river.

By contrast with 4, the example:

Happiness flooded my heart.
*My heart was flooded by happiness.

shows flood as a synonym of fill, and by is replaced by with.[10]

With a number of the verbs cited a true agent may be introduced, as in John fills the glass vs. The water fills the glass, which of course then passivizes: The glass is filled (with water) by John. But instrument as well as agent answers to the semantic intent of the speaker. In:

A mist filled the valley.
*The valley was filled by a mist.
*The valley was filled with a mist.
*The valley was full of a mist.

the verb fill is merely a colorful substitute for a preposition: a mist was all over the valley. 'Mist' as an instrument is almost as doubtful as 'mist' or some other entity as agent.

Similar cases are those of verbs used in a presentative sense. The special syntax of "bringing on the scene" is well known: Comes now John Jones plaintiff, and deposes; Down came the rain; Out of the dank waters emerged a hand. If normal word order is kept, the newly introduced item is accented and the rest of the sentence is lowered in pitch: On the farther shore a hand beckoned; Toward dawn an eerie cry awakened me. Even though the passive is a natural in such cases (I was awakened by an eerie cry) since it puts the presented item in end position, if the verb itself is presentative, i.e., means in some sense 'to put on the scene', the passive is excluded. It makes no difference whether the verb is accompanied by a preposition or not:

Suddenly the heroine crossed the stage.
*Suddenly the stage was crossed by the heroine.

Suddenly the héroine walked on the stage.
*Suddenly the stage was walked on by the heroine.

At that moment Hámlet entered (came on) the scene.
*At that moment the scene was entered (come on) by Hamlet.

The same restrictions are found in more abstract expressions of presence-absence:

A slave implies a master. ('If there is a master there is a slave.')
*A master is implied by a slave. (OK A master is implicit in a slave, in there being a slave.)[11]

This evidence implies a plot.
A plot is implied (made real) by this evidence.

Our study involves chemistry.
*Chemistry is involved by our study.

Mary involved John in her scandal.
John was involved by Mary in her scandal.

The problem concerns arithmetic.
*Arithmetic is concerned by the problem.

Your attitude concerns me.
I am concerned by your attitude.

With these there is something of an underlying 'in': a question of chemistry in the study, a question of arithmetic in the problem.

One form of location, proximity, does passivize -- as if being close produced an effect:

The wall was abutted by a large shed.
The garden is adjoined by a graveyard.
The tenth is preceded by the ninth.
The little church was stood next to by an office building.
*The little church was stood in front of by an office building.

Where a spatial relationship expresses a use, the corresponding verb + preposition passivizes just as the verb use does. Thus for a circus act, This stool is jumped from, that one is jumped to, and that one is leaned on. But *The door was leaned on by the postman while I went for some change.

I have emphasized presence-absence because it is more subtle than the other spatial relationships such as 'to', 'from', 'over', etc. The contrasts are obvious in the following:

1. A tree overhangs the stream.
2. *The stream is overhung by a tree.
3. A beetling cliff overhangs the stream.
4. The stream is overhung by a beetling cliff.
5. Our house overlooks a broad expanse of lake.
6. *A broad expanse of lake is overlooked by our house.
7. An awesome mountain overlooks the peaceful valley.
8. The peaceful valley is overlooked by an awesome mountain.
9. George turned the corner.
10. *The corner was turned by George.
11. George turned the page.
12. The page was turned by George.
13. Alpha begins and omega ends the series of letters in the Greek alphabet.
14. *The series of letters in the Greek alphabet is begun by alpha and ended by omega.
15. A machine began the work and ended it.
16. The work was begun and ended by a machine.
17. The train neared the station.
18. *The station was neared by the train.
19. We neared the danger cautiously.
20. The danger was neared cautiously.
21. A delicious feast concluded the celebration.
22. *The celebration was concluded by a delicious feast.
23. The chairman concluded the celebration.
24. The celebration was concluded by the chairman.
25. A cherry topped the cake.
26. *The cake was topped by a cherry.
27. The cake was topped by one lone cherry.
28. A 30-foot pole tops the building.
29. The building is topped by a 30-foot pole.
30. The two customers entered the store.
31. *The store was entered by the two customers.
32. The two thieves entered the store.
33. The store was entered by the two thieves.

If an exegesis is needed, the meaning of transitivity can be paraphrased, e.g., 4 'threaten', 8 'dominate', 20 'dare', 33 'burglarize' (compare the

legal breaking and entering). In 29 the pole is the finishing touch, as the cherry is, ludicrously, in 27 -- it perfects the ensemble. In the opposite cases prepositions can be made explicit: 2 and 6, 'over'; 10, 'around'; 14, 'at'; 18, 'toward'; 22, 'at'; 26, 'on'; 31, 'into'.

Some verbs appear to have no contrasting counterpart on the side of true transitivity:

The U.S. neighbors the northern border of Mexico.[12]
*The northern border of Mexico is neighbored by the U.S.

The plane departs Boston at 6 P.M.
*Boston is departed by the plane at 6 P.M.

The thief escaped the jailer.
*The jailer was escaped by the thief.

The reason escapes me.
*I am escaped by the reason.

He outlived all his friends.
*All his friends were outlived by him.[13]

Others of this group may be transitive under some conditions:

1. George shunned publicity.
2. *Publicity was shunned by George.
3. Publicity should be religiously shunned.

In 3 we are told how publicity is to be handled; in 2 we are merely told that George kept away from it. Somewhat the same is true of:

1. They missed the train.
2. *The train was missed by them.
3. How many times does that train have to be missed before they fix the schedules?
4. The two men shared the limelight.
5. *The limelight was shared by the two men.
6. It is only fair for the limelight to be shared.

In 2, 'on' or 'to' is involved; in 5 it is 'in'.

There are accordingly many verbs that are borderline, and many factors that influence our judgment. One is relative size. It is ridiculous to think of the Pacific being affected by a raft, but a fleet is a different matter. Similarly one visitor against many, person-to-group versus person-to-person, etc.:

The Kon-Tiki has twice sailed the Pacific.
*The Pacific has been sailed twice by the Kon-Tiki.
The Pacific has been sailed by the mightiest fleets in history.

I often visit the capital.
*The capital is often visited by me.
The capital is visited by many tourists every year.

He frequents the Blue Moon.
*The Blue Moon is frequented by him.
The Blue Moon is frequented by the off-Broadway crowd.

My brother has lived in Chicago.
*Chicago has been lived in by my brother.
Chicago has been lived in by generations of immigrants.

A bug crawled on him.
*He was crawled on by a bug.
He was stepped on by an elephant.

George left the city.
*The city was left by George.
The city was left by all the male inhabitants.

The bird alighted on the house.
*The house was alighted on by the bird (OK by the helicopter).
The twig was alighted on by the bird.

Smith owns that property.
?That property is owned by Smith.
That property is owned by Smith and Wesson.

Size may be temporal as well as spatial -- action over a period of time increases the potency of an agent:

*My fur coat is clung to by dust.
This kind of fabric has a tendency to be clung to by dust.

Relative size is only a manifestation of relative power. While we can hardly say *The Army was deserted by John, there is no problem with The Army was deserted by its commander-in-chief. There may be an explicit reference to power:

*The mike was yelled into by the speaker.
This mike is too delicate to be yelled into by somebody with a voice as loud as yours.

Position is also a factor; being over something is dominant, being under is the opposite:

The formation is overlain (*underlain) by an almost impenetrable layer of hardpan.

Where the nature of the verb raises a doubt, other factors intervene to sway the possibilities one way or the other. Verbs of perception are particularly sensitive. At first blush one wonders how such a verb as see can passivize at all if my hypothesis is correct, for how can anything be affected by merely being seen? And indeed the passive is impossible in situations like:

Do you see that table?
*Is that table seen by you?

John feels the wind.
*The wind is felt by John.

Yet we easily metaphorize see and related verbs to the sense of perceive: Is that table perceived by you? And events in the past are readily reported: Was that table seen by you? Figuratively, it appears, we "capture" the object as a sense datum. The delicacy of the balance can be seen in a pair like:

The two witnesses saw the man, and promptly testified.
The two boys saw the man, and promptly ran away.

where both are normal in the active voice, but not in the passive:

The man was seen by the two witnesses, who promptly testified.
?The man was seen by the two boys, who promptly ran away.

Conceptually, the witnesses' seeing the man affects the man; the boys' seeing the man affects the boys. Our surprise at the ease with which see passivizes is not a fact of grammar but an illusion created by the great semantic range of that verb.

Instrumental passives are influenced by a strange combination of "predictability" and "custom of use". As we saw earlier, predictability is allied to lexicalization: to cut, for example, is more conventionally associated with knife than it is with shears. Other examples of highly predictable associations are write with pen, plow with plow, eat with knife and fork. Examples of less predictable associations are steal with burglar's tools, walk with cane, and open with can-opener. So we get:

My knife is being cut with right now.
?My shears are being cut with right now.
My pen is being written with right now.
*My burglar's tools are being stolen with right now.
My plow is being plowed with right now.
*My cane is being walked with right now.
My knife and fork are being eaten with right now.
*My can-opener is being opened with right now.

Presumably our quickness in associating knife with cut is what makes the passive -- with its less usual order -- more easily interpretable there than it is with shears. But when the context is one that refers to custom of use, all the examples are either fully acceptable or considerably improved:

These shears have never been cut with -- they're too sharp.
This cane has never been walked with; there's not a scratch on it.
I don't like to have my can-opener opened with by somebody else.

Though most of the examples I have cited in the second half of this paper have used either simple verbs or monotransitive prepositional verbs, ditransitives obey the same principles. A couple of illustrations should suffice. It is normal, for instance, to service a table by laying a

tablecloth on it, but a book on a table is merely there. On the other hand, a shelf is made to be loaded with books, not ordinarily with tablecloths:

Which tables have been laid tablecloths on?
*Which tables have been laid books on?

Which shelves have been put books on?
*Which shelves have been put tablecloths on?

The last example suggests ludicrously that the shelves have been provided with tablecloths. Although all four examples are lowered in acceptability by the addition of the definite article (one usually "services" a table with tablecloths, indefinite, not with tablecloths, definite), the relative acceptability remains the same and the contrasts show more clearly:

?Which tables have been laid the tablecloths on?
**Which tables have been laid the books on?

?Which shelves have been put the books on?
**Which shelves have been put the tablecloths on?

The English passive has established a semantic distinction between true transitivity and spatiality. It has its own meaning, which the speaker calls on to accord with his intentions. Given the semantic features of most verbs, we can safely predict whether a speaker will elect to use them transitively or not, and in this sense transitivity may be regarded as a feature <u>of</u> those verbs. But with many simple verbs and most prepositional verbs, transitivity is not a feature of them but a feature <u>in</u> them, put there on occasions of use. The difficulties that linguists have had with prepositional verbs have been due partly to too much insistence on immanent rather than transcendent features, and partly to lumping all cases of ungrammaticality together and failing to separate those due to violations from those due to difficulties of processing. This assessment is only a particular way of saying that constructions with prepositional verbs must meet the same two tests as all other constructions: they must sustain a meaning, and they must be interpretable.

Does transitivity as I have defined it have any reality in our minds? A list of examples from this study was presented without explanation to a group of 70 college freshmen with instructions to find a reason for acceptability or its lack. The replies were random except in one respect: 10% approximated the wording "passive if really acted on". As with most such distinctions, not many untrained people can bring them to the surface. The fact that a few can is proof enough that the mechanism is there and functioning.

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Footnotes

[1]The negative of understand is normal: Her husband doesn't understand her (emotionally).

[2]Jespersen 1928: § 15.7_3 words the rule similarly, but does not give it quite the blanket coverage of the others: "In nearly all the modern sentences we have set phrases consisting of verbs with a short object without any qualifying words". One of his examples, from Walpole, can hardly be considered a set phrase, though it does involve the verb make, which comes close to being a compounding element: I was to be made a fighting ground of.

Mustanoja 1960: 440-1 sets the origins of both the John was given a book and He was laughed at types around 1300. Jespersen 1928: § 15.7_1 cites an example of prepositional verb with retained accusative from Sir Thomas More: Their bodies were layed hold on.

[3]If the lexical combination is hermetic, it makes no difference what elements go with the verb. They may be adjectives as well as nouns: made free with, made light of, and made sure of can replace made use of.

[4]See Bolinger 1972. The version The subjects which were given examinations on... suggests "new to context" but also strikes me as a trifle incongruous. I now believe that the preference for that in restrictive clauses and for which and who in nonrestrictive ones is a further manifestation of a lexical difference and not merely a transformational difference. Restrictive clauses identify their antecedents by pointing back to previously mentioned information (the man that complained = 'the man associated with the known complaining'). Nonrestrictive clauses introduce new information (the man, who complained). When which or who appears in a restrictive clause, it is as if to refresh the hearer's memory, not to assume it.

[5]The factor of interpretability that I have tried to develop here can be explained fairly well by the information-processing model proposed by Gough 1972 in his description of reading. He posits a primary memory in the shape of "a small-capacity storage system where 4 to 5 verbal items are maintained for a matter of seconds.... When the PM is full, an item in residence must be displaced if a new item is to enter" (300-1). When the contents of this PM are processed (a structure assigned, an interpretation given) they pass to secondary memory and PM is cleared for the next batch. What this means is that "any sentence whose initial words exceed the capacity of PM before they can be understood...will prove incomprehensible. This is just the case with sentences self-embedded to a degree of 2 or more" (302). It would also appear to be the case with at least some of the problem sentences that I have listed. In the example *I don't like to be blamed mistakes on the model says that the

portion I don't like to be blamed represents approximately an optimal load for PM. There would be a strong temptation to assign it a structure, which would of course be the wrong one. Gough's model is a bit too simple to explain why the infinitive, interrogative, and other structures are less susceptible to this limitation, and I suspect that we must look for some mechanism which warns the hearer (or the reader) not to close the books on PM until more data come in. The for of the infinitive, the position of the interrogative word, and other cues would serve to trigger this mechanism.

[6]The possibilities of literary invention cannot be gone into here: I felt my gaze held as in a vise by his magnetic eyes.

[7]The understanding of this should not be sidetracked by the irrelevancy that destination is concrete and conclusion abstract. House in 4 is as concrete as Chicago in 2. The most we can say is that if the verb contains an explicit preposition and takes an abstract object, the meaning of the preposition is apt not to be the literal one: They went into the problem, The problem was gone into. In such a case, verb and preposition form a unit. But it need not be so, e.g., He went into the dispute with both arms flailing is a "literal" use of go into as a new figurative extension. And while the chances are good that with a concrete object the preposition will have just its spatial value, examples like 4 show that transitivity can be added.

[8]The verb contain has acquired a curious apparent passive, I think by analogy, in examples on the order of More particles are contained in the first receptacle than in all the others combined. That this is not a true passive can be shown by the impossibility of modals:

*More particles will be contained....
*More particles can be contained....

and also by the impossibility of adding an agent: *More particles are contained in the first receptacle by John than.... My guess is that the analogy is with the type More particles are kept (stored, held)... to which an agent may be added but usually is not. The value of contained is that it refers to capacity and thus contrasts with the transitory or irrelevant location implicit in simple be. More particles are contained in the first receptacle refers to the amount of particles and describes the receptacle; More particles are in the first receptacle refers to particular particles and describes them, not the receptacle.

[9]I would put down as an idiom the legalistic She is survived by her husband and ten children. The passive is more usual than the active here.

[10]The person, not the heart, is viewed as the affected thing: I was cheered by a heart full of happiness is normal, as is My heart was cheered by the news.

[11]Possible reading: 'There being a master is implied by there being a slave' -- if the subject is taken in the 'there being' instead of 'there is' sense. For this reading, a heavy disjuncture is required after the subject, and slow rate of speech.

[12]Example s.v. neighbor in the Merriam Third.

[13]This example shows after in a temporal sense. I use the term spatiality to cover time as well as space. The two dimensions are similarly quantified and share most of the same prepositions.

THE PLACE OF GENERATIVE PHONOLOGY[1] IN THE HISTORY OF LINGUISTICS

by

Fred C. C. Peng

International Christian University

0. Introduction. For quite some time, I have been wanting to place generative phonology in the history of linguistics; I am curious as to where it stands and how well it fares out relative to other phonologies.

An assessment of this kind is perhaps best presented in the form of a book, sketching first the major trends, step by step, then coming to grips with the assessment. Unfortunately, at the present moment, I am in no mood to write a book of this magnitude. In the absence of such a book, the next best presentation is probably an article; however, it entails having a body of published materials documenting the main issues raised by the various schools of thought. Makkai's Phonological Theory: Evolution and Current Practice (1972) contains a sufficient number of such materials and certainly fulfills the role. My view of generative phonology, from the viewpoint of history, will therefore be based in the main on these materials, supplemented by other sources of information whenever deemed necessary.

That an article of this nature should appear at this stage of the game, limited though it may be, is I think rather timely. There are two reasons. One, I sense a revival of interest in the history of our science among linguists at large. My reasoning in this regard is that the current practice in phonology, generative phonology in particular, has changed the nature of the science of language and made it less and less relevant to man's verbal behavior vis-a-vis his social contexts and cultural environments. And, two, generative phonologists have gotten themselves trapped in a state of confusion, such as the mixing of diachrony and synchrony pointed out in Peng (1972b:269-70) and, therefore, must be told that they are repeating the same kind of extremism the Junggrammatikers had committed many decades ago and that they have misled the entire academic community in linguistics to a dead end.

To avoid discussing the history of generative phonology itself, which is best presented in the yet-to-be-written book, I shall single out four topics for a more concentrated evaluation. These topics are: (1) The Substances of Phonology; (2) Differentiation of the Substances; (3) Relations and Patterning; and (4) Status of the Phoneme. To these will then be added a conclusion which will pronounce the verdict.

1. The Substances of Phonology. What are the substances of phonology with which linguists must deal? Very often, when this question is asked, the answer is "sounds," that is, the noises made by humans through the vocal tract. This simple question should not have been asked here, because as linguists we all know or are supposed to know

the answer. Such is not the case, however, in generative phonology. For some strange reasons, which I will speculate on below, generative phonologists have chosen only certain noises for their frame of reference and completely paid no attention to, or are totally inept to account for, other noises which linguists using different frames of reference have comfortably dealt with for decades.

To present this sharp contrast between generative phonology and other approaches, say, taxonomic phonology, for the sake of history, I shall follow the traditional division of those noises that come out of the vocal tract as speech into segmental and suprasegmental. A cursory review (Peng n.d.) of Makkai's anthology (hereafter PT)[2] suggests that all approaches in phonological theory thus far developed, with the sole exception of generative phonology, have adhered to this dichotomy. This view, to be sure, is shared by other scholars, for instance, von Raffler Engel who, in her recent paper (1974), has pointed out that Chomskyan radical nativism is based on a logical scheme strictly derived from certain aspects of syntax exclusive even of intonation contours.

As early as 1957, however, in his review of Jakobson et al. (1956), Chomsky himself, interestingly enough, commented on investigation of prosodic features and clarification of their relation to the remainder of the phonological and grammatical structure and proclaimed that "There is some discussion of this problem, but it is very sketchy, and it remains to be seen whether the proposals made can do justice to the full range of intonational phenomena" (PT:350). This is evidence that during this period, although generative phonology was in its infantile stage, if not unheard of, Chomsky was aware of the dichotomy and did show signs of striving for a fuller description, segmental as well as suprasegmental.

Nonetheless, transformational grammarians, then headed by Chomsky and Halle, took no heed of this very flaw pointed out by Chomsky himself. Ferguson (1962), in his review of Halle (1959), added this criticism: "The suprasegmental phenomena are given only the most cursory treatment" (PT:376).

Of course, suprasegmental noises are not limited to intonations; stresses, transitions, and lengths, etc., are included. But, strangely enough, nothing since then has come out on intonations and tones that is of theoretical importance in generative phonology.[3] Stockwell's "The Place of Intonation in a Generative Grammar of English" (1960) is the first and only one available within the framework of transformational generative grammar. But note that it is not at all period detached from structural linguistics (or taxonomic phonemics) distinctly reflected in Wells (1945) and Trager and Smith (1951) which it takes for granted. Presumably, this is why Stockwell's article has never been accepted by generative phonologists.

More strange, however, is the fact that even in as late as Chomsky

and Halle (1968), Harms (1968), and Schane (1973), all suprasegmental noises generative phonologists have ever cared to touch upon are stresses in English. I wonder why they are so scared of intonations and tones in general. I have commented on this peculiar point before (1967) and, therefore, will recapitulate it in brief here for further consideration.

There are, essentially, three problems, if not more, which need to be taken into account, when analyzing suprasegmental noises such as intonations and tones within any theory: (1) the distinction between the tones of a Chinese language, say, Mandarin and those of another tonal language, say, an African tonal language; (2) the distinction between tones, such as those in Mandarin, and intonations, such as those in English; and (3) the distinction between tones, such as those in the African tonal language, just in case the distinction in (1) above exists, and intonations, such as those in English. I shall discuss in the following the first and second problems, leaving the third one unexpressed.

The distinction between the tones of Mandarin and those of an African tonal language, say, Kpelle must be carefully taken into consideration by any theory. Certainly, we cannot allow the tones of Kpelle to be characterized in the same way as those of Mandarin (if there is such a way) and vice versa, on the grounds that the tones of Mandarin are segmental, whereas those of Kpelle are suprasegmental--in the technical sense of the terms.

If, for any conceivable reason, the distinction should not be given proper attention (in other words, should the two tonal systems be coalesced and treated alike), it must follow that no adequate description will ever be arrived at and it is not at all difficult to imagine the consequences. Compare, for example, the following:

Kpelle[4]

píli	'jump'	è pìli	'he jumped'
kula	'go out'	è kùla	'he went out'
pêlaŋ	'get down'	è pèlaŋ	'he got down'
tuâŋ	'move over'	è tùaŋ	'he moved over'

Mandarin[5]

t'iao^4 ch'i^3 lai^2	'jump'	t'a^1 t'iao^4 ch'i^3 lai^2 (le)	'he jumped'
tsou^3 ch'u^1 ch'ü^4	'go out'	t'a^1 tsou^3 ch'u^1 ch'ü^4 (le)	'he went out'
hsia^4 lai^2	'get down'	t'a^1 hsia^4 lai^2 (le)	'he got down'
nuo^3 kuo^4 lai^2	'move over'	t'a^1 nuo^2 kuo^4 lai^2 (le)	'he moved over'

Consider now the tonal shifting in Kpelle. It entails not only tone sandhi but also affixation. For instance, the change from /^/ to /-/[6] in 'move over' is probably a sandhi phenomenon alone, but the change from /-/ to /ˋ/ in 'move over' is a sandhi phenomenon signaling a certain affixation, namely, the past tense. In Mandarin, however, while there is definitely no affixation involved, as far as tonal shifting is concerned, there are sandhi phenomena which, in the examples cited, involve only the third tone, and the optional neutralization of the tones of lai, ch'ü, and kuo, the tone sandhi, in this case, being realized merely phonetically (i.e., allophonically, in its technical sense).[7]

The distinction between tones, such as those in Mandarin, and intonations, such as those in English, must also be given proper attention. Furthermore, the distinction must be such that not only should the difference between a Mandarin utterance consisting of one sentence or more be appropriately accounted for, but also the difference between a Mandarin utterance consisting of one and only one word, whatever a word may be in each language, be equally appropriately accounted for. The ground upon which this distinction is based is similar to the aforementioned; that is to say, the tones in Mandarin are segmental, whereas the intonations in English are suprasegmental. As an example, compare the following:

English

/^{2}ày wênt hôwm ^{3}yésterdiy^{1}↓/	'I went home yesterday'
/^{3}giv it tùw im^{1}↓/	'Give it to him'
/^{2}hùwz ^{3}gówiŋ^{3}↑/	'Who's going?'
/^{3}míy^{1}↓/	'Me!'
/^{3}míy^{3}/	'Me?'

Mandarin

wo^{2} tsuo^{3} t'ien^{1} huei^{2} chia^{1} ch'ü^{4}	'I went home yesterday'
kei^{3} t'a^{1} nei^{4} ko	'Give it to him'
shei^{2} ch'ü^{4}	'Who's going?'
wo^{3}	'Me!'
wo^{3}	'Me?'

Regarding intonation and tone sandhi in these utterances, two observations are in order: First, consider intonation. In this case, observe that there is a substantial change between a statement (or a command for that matter) and a question in terms of English intonation, involving the shift from pitch 1 to pitch 3, whereas there is little change between a statement (or a command) and a question in terms of Chinese intonation, except that the fourth tone in ch'ü and the first tone in t'ien of the first item may optionally be neutralized, becoming the so-called atonic, but that while the fourth tone in ko of the second item has obligatorily been neutralized, the fourth tone in ch'ü of the third item must remain unchanged.

In the case of miy, which is a one-word utterance, the pitch contour may change up and down, depending upon whether the item concerned is a statement or a question, but in the case of wo, which is also a one-word utterance, the pitch contour remains essentially the same, irrespective of whether the item concerned is a statement or a question.

Second, consider tone sandhi. In this case, observe that while no tone sandhi exists in the English examples, certain tone sandhi have taken place in the Chinese examples. Most noticeable of all, the third tone in wo has become the second tone in the first item because of another third tone[8] that follows it (but has remained unchanged elsewhere).

The materials presented above are not raw but analyzed data based upon taxonomic phonemics. Why is it then that taxonomic phonemics has been able to account for tones and intonations so well but generative phonology has yet to come close to them? Three reasons are conceivable, which may explain why generative phonologists have shied away from tones and intonations such as those which taxonomic phonologists have so skillfully analyzed.

One, as will be shown later, generative phonologists have allegedly abolished the concept of the phoneme (cf. § 4.1) and, therefore, they can no longer, with a clear conscience, claim the use of the phoneme which is the key to the analysis of tones and intonations in taxonomic phonemics, unless they can come up with a disguise, such as that which they have called the systematic phoneme, to replace the phoneme. They think such is the case in segmental phonology, a point that will be shown quite to the contrary later. But when it comes to suprasegmental phonemes, such as Kpelle tones and English pitches, generative phonologists have not been and will never be able to find fictitious substitutes other than those taxonomic phonemes that have already been established.

Two, the supposedly tight relation between the phonological component and the syntactic component of transformational generative grammar has been a hindrance to any progress in the study of tones

and intonations. I am even in doubt that the so-called tight relation that has been advocated is really kept up, in view of the recent works in generative phonology listed further above. At any rate, let us suppose that there is such a tight relation. It must follow then that while there is no need, in the case of Mandarin, to introduce the tones as separate formatives (or morphemes) by way of the syntactic component of its grammar, there is such a need, in the case of Kpelle, to introduce the tones, or tonal combinations (or both), as separate formatives (or morphemes), also by way of the syntactic component of its grammar, assuming that generative phonologists are willing to accept the phoneme as has been defined by taxonomic phonemicists. Similarly, a pitch in English should not be introduced as a formative, although a sequence of pitches in English, hence English intonation, should. It would be worse, if we push further to ask for a distinction, to serve as the input to the phonological component, between Kpelle tonal formatives and English intonational formatives.

Three, in the case of English (or any other European language) wherein both stress and pitch phonemes exist) generative phonology is further complicated because of its preoccupation with stresses at the expense of pitches. Note that stresses in English, according to the most recent publications on generative phonology alluded to earlier, are the only place I know where the tight relation can be truly demonstrated. Other devices, such as the use of morpheme features, e.g., [+Sino-Japanese] mixing history and physiology in one bunch, as it were, have only burdened the already chaotic notation in generative phonology. Unfortunately, however, the cyclical rules of English stress patterns are formulated at the expense of English intonation patterns; there seems to be no more room for intonations once the cyclical rules have been applied, a built-in flaw of generative phonology that contradicts the actual facts of the English language.

2. Differentiation of the Substances. If generative phonologists have, to their detriment, ignored suprasegmental substances, what about segmental substances? They have claimed so many merits over the past decade in this connection that johnny-come-latelys often think that non-generativists had not done anything right. Let us, therefore, take a closer look at the actual publications dealing with such substances to see how well generative phonology fares out compared with other phonological theories. To focus the topic more succinctly, let me suggest three points for further discussion: (1) psychological reality, (2) phonetic similarity, and (3) neutralization.

2.1. Psychological Reality. Most linguists would agree that if sounds are differentiated enough in a language, native speakers are aware of their differences, for instance, vowels versus consonants, or between [p] and [k], or [i], [u], and [a]. They would further agree that sounds which are differentiated in one language in a certain way are not necessarily differentiated in the same way in another language; that is to say, native speakers of these

languages may have different ways of differentiating the sounds that are similar in their languages, thereby possessing differing degrees of awareness. This kind of awareness is probably what Sapir called psychological reality or phonemic intuition (1933). And the differentiated sounds are phonemes. Examples: "To the native speaker and hearer, sounds (i.e., phonemes) do not differ as five-inch or six-inch entities differ, but as clubs and poles differ." "... I have come to the practical realization that what the naive speaker hears is not phonetic elements but phonemes." "It is exceedingly difficult, ..., to teach a native to take account of purely mechanical phonetic variations which have no phonemic reality for him" (PT:23).

But Sapir's line of psychological differentiation is much more complex than has been stated. For one thing, according to Swadesh (1935), "the behavior of native speakers of a language proves a psychological reality for the phoneme (this is not the same as saying the phoneme is a psychological reality" (PT:43). For another thing, as Swadesh has also pointed out, the behavior of native speakers of a language proves a psychological reality for not only those phonemes which do not change morphophonemically but those phonemes which undergo morphophonemic alternations as well (PT:43). The latter point is particularly important, as it is not the same as saying the behavior proves a psychological reality for morphophonemes.

An example will suffice to demonstrate this important point. The l in light (versus right) is a phoneme in Sapir's sense for which native speakers of English have a psychological reality in their behavior; this behavior also proves another psychological reality for f in fine and v in vine. But note that while f and v undergo morphophonemic changes, as may be evidenced by wife and wives, l never does; no one would dare suggest then that the behavior of native speakers of English prove a psychological reality for the morphophoneme F (subsuming f and v).

If Sapir's psychological reality illustrated above is employed as a yardstick to measure differentiation of the substances in question, something quite interesting happens. Interesting, because Chomsky (1964), in urging linguists to return to Sapir's psychological reality (PT:416) which he exploits as "systematic phonemics," does not really know what he is talking about. For what he showed to be systematic phonemics turns out to contradict psychological reality.

One example that has appeared repeatedly in the literature to show how wrong the biuniqueness condition is comes supposedly from Halle (1959) as indicated by Chomsky (1964). This very same example is also quoted by Johns (1969) referring once again to Halle. For the sake of historical accuracy, let me say that I have failed to locate this famous example in Halle (1959), although the argument against biuniqueness using Russian obstruents as an example can be found in Halle (1959:22-23) which Lamb (1966) has already torn apart

(PT:612). Be that as it may, I shall cite this ghostly example once more to prove that Chomsky is clearly in error according to Sapir's yardstick which he himself has invoked.

(38)	I	II	III
	d'at, l,i	d'at, l,i	d'at, l,i
	d'at, bi	d'ad, bi	d'ad, bi
	z'eč l,i	z,eč l,i	z,eč l,i
	z'eč bi	z,eč bi	z'ej bi

In line with Chomsky (PT:413), "The four forms in Column I are given in systematic phonemic representation and in Column III in systematic phonetic representation." Column II should thus be eliminated, in Chomsky's opinion. Note that what he calls systematic phonemic representation contains no differentiation between the two palatalized obstruents nor between the two alveopalatal obstruents but that each pair is clearly differentiated in what he calls systematic phonetic representation.

Given this much data, as no others are provided, Chomsky is absolutely right, and he did not have to call Column I systematic phonemics and Column III systematic phonetics in order to eliminate Column II. Because taxonomic phonemicists would surely eliminate Column II themselves, as [t,] and [d,] in keeping with [č] and [j] are in complementary distribution. Consequently, taxonomic phonemicists would choose Column I as their phonemic solution, just as Chomsky has; this is standard practice in any workbook available on taxonomic phonemics, such as Pike (1947a) and Gleason (1961). The difference between taxonomic phonemics and systematic phonemics is in sight when and only when extra information is added, such as t,'el "body" (gen. pl.) and d,'el "deed" (gen. pl.), because of the fact that now t,/d, contrast with each other while č/j do not, as Chomsky himself has already pointed out. Does this mean, then, that native speakers of Russian are not aware of the differentiation between t, and d, just as they are not aware of the distinction between č and j, if Chomsky seriously wants to stick to Sapir's psychological reality? In other words, if Sapir's yardstick is applied here, is Chomsky sure of the proportion t,:d, :: č:j in the context of Voiced Obstruent and/or Initial Position, which becomes t,:t, :: č:č intuitively?

Here, Chomsky has a dilemma. If yes, and I suspect that this would be the answer based just on the three columns, I wonder what he would say regarding the contrasting pair in the added information, since the differentiation of č/j does not appear initially! Certainly, native speakers of Russian, including Halle, though not Chomsky, would differentiate t, and d, (given the added data) as a club from a pole but could not do so, no matter what, with regard to c and j, because they are only five-inch and six-inch entities.

This fact is recognized by Halle himself when he distinguishes

between fully specified segment types and incompletely specified segment types (1959:37). Thus, č/j become one fully specified type č because only č occurs initially, whereas t,/d, are two incompletely specified segment types, so Halle symbolizes them by starring the cognate fully specified counterparts, e.g. *t and *d (1959:47-48), because both t, and d, occur initially. That is to say, to the Russians, t, and d, are much more differentiated, cognitively and psychologically, than č and j, contrary to Chomsky's unfounded claim. From these observations it must follow that Chomsky was the one who obscured this important fact in Column I; his rule (PT:413)

(40) Obstruent → Voiced in the context:
____ Voiced Obstruent, except for c, c, x

conceals, to say nothing else, if not violates, the otherwise perfectly clear native (and I must hasten to add, phonemic) intuition.

If, on the other hand, Chomsky's answer is no, having come to realize the existence of initial t,/d, mentioned earlier, then he has no business dismissing Column II, because it is Column I that is wrong not II which matches Sapir's psychological reality very well and reflects native speaker's intuition. The reason for Chomsky's failure is simple. In Column I t,/d, are lumped into one morphophoneme t, (similar to F mentioned above for wife and wives) for which native speaker's behavior does not prove a psychological reality when, in fact, the psychological reality exists for t,/d, because of words like t,'el and d,'el but not for č/j.

In light of the preceding evidence against generative phonology, it is highly improper, presumptuous, and misleading, to say the least, if not totally insulting, for McCawley to state that "The name of Edward Sapir has been invoked many times recently as a precursor of modern generative phonology (1967:106). Sapir never shied away, in the first place, from tones and the like; in fact, unlike generative phonologists, he has taken on the anlyses of many tonal languages in his writings, one of which is Sarcee, the very language employed to illustrate his point on psychological reality. In the second place, as Swadesh has clearly explicated, Sapir's idea of the psychological reality is not, and never has been, meant to be for morphophonemes, such as the t, in Column I, but rather for t,/d, in Column II and t,'el/d,'el.

2.2. Phonetic Similarity. When sounds are differentiated only to the degree just below native speakers' awareness, unless they are well-trained phoneticians themselves who can tell minute differences, the differentiation thus made is no longer significant. Linguists then begin to speak of phonetic similarity. But similarity can be very misleading, a fact that has caused so much argument in the history of linguistics, as has been amply demonstrated by Haugen and Twaddell (1942) who refer to phonetic similarity and phonetic interrelationship as "dangerously loose terms" (PT:94).

Two points are worth discussing here. First, similarity is misleading in the sense that sometimes other facts, such as relations and patterning, interfere, thereby making the judgement difficult. One famous example in this regard appears in the dispute between Swadesh (1935) and Twaddell (1936) over the following data (PT:42 & 45).

	Norm in open syll.	Norm in closed syll.
(a)	[i]	[ɪ]
(b)	[e]	[ɛ]
(c)	[ɛ]	[æ]

Swadesh wanted to regard [e]/[ɛ] in (b) as phonetically similar (PT:42) but Twaddell suggested that if similarity meant anything, why not the identical pair [ɛ]/[ɛ], an attempt to discredit phonetic similarity (PT:45). This is a hopeless case, as the data were artificially created by Swadesh.

Second, a more serious problem regarding phonetic similarity, which is real and not at all artificial, is the often cited example of the flap in such pairs as decided/delighted, rider/writer, bidder/bitter, etc. That this is misleading is because phonetic similarity is disguised with a symbol from which Chomsky has drawn erroneous conclusions. In Chomsky's illustration of decided/delighted (PT:405) and rider/writer (PT:410) which is repeated in (PT:537), for instance, he assumes a d/t distinction in what he calls systematic phonemic representation, but no distinction, i.e., an alveolar flap [D], in systematic phonetics. (No one knows, and nowhere has Chomsky ever said, whether this [D] is supposed to be voiced or voiceless.) All Chomsky has ever claimed is that decided is disa·yDɨd and delighted, dilayDɨd or that rider is ra·Dɨr and writer, rayDɨr. Clearly, then, similarity of the two flaps in each pair is disguised to the extent of complete identity with the symbol [D].

But there is evidence that such a disguise is a farce on Chomsky's part. First, let me point out that phonetically the two flaps may be differentiated, one being voiced and the other, voiceless. Smalley (1962) symbolizes them as [ď] and [ť], respectively. Since Chomsky has never run any laboratory test to substantiate his [D], I have enough ground on which to claim that what he heard is actually [ď] for decided and rider but [ť] for delighted and writer. There is a chance that native speakers of American English would fluctuate between the two flaps, say, 70% for [ď] and 30% for [ť] when uttering decided and rider, in citation or in context, but the other way around when uttering delighted and writer.[9] If Chomsky is willing to pick at random a sample from among a sizeable group of American speakers to test these words, he will no doubt find that his [D] is under-differentiated and totally unfounded in phonetic terms.[10]

Suppose that [ď] and [ť] are the flaps with which we must be concerned. Immediately, systematic phonemics must be reduced to ashes, because its existence depends entirely on the fictitious [D]; once [D] is removed, for a good reason, systematic phonemics evaporates, for /disaydɨd/ and /dilaytɨd/ are traditional phonemic transcriptions, pure and simple, wherein /d/ and /t/ are the taxonomic phonemicization of [ď] and [ť], respectively. Also gone for good are Chomsky's ordered rules (PT:405)

(28) a→a· in the context: _ (Glide) Voiced

(29) [t,d]→D in the context: Stressed Vowel _ unstressed Vocalic

simply because in view of the clearly differentiated [ď] and [ť], the rules so presented are sheer nonsense. Note in particular that (28) is merely a formulaic parroting of Sapir's penetrating observation (1925): "Thus, in (American) English there is a perceptible difference in the length of the vowel a of bad and bat, the a-vowel illustrated by these words being long or half-long before voiced consonants and all continuants, whether voiced or unvoiced, but short before voiceless stops. In fact, the vocalic alternation of bad and bat is quantitatively paralled to such alternations as bead and beat, fade and fate. The alternations are governed by mechanical considerations that have only a subsidiary relevance for the phonetic pattern. They take care of themselves, as it were, and it is not always easy to convince natives of their objective reality, however sensitive they may be to violations of the unconscious rule in the speech of foreigners" (PT:16). Note further that (29) is worthless to begin with, because it cannot even hope to account for such cases as cotton, kitten, button, rotten, written, threaten, sweeten, versus sadden, sudden, widen, maiden, burden, Jorden,[11] and Haydn, and numerous others.

Suppose, on the other hand, that the flaps are not [ď] and [ť]. There are only two choices left: both [ď] or both [ť]. I have yet to find any document, well founded empirically, to support the second. So, let us take the first. A qualification must follow, however. That is, the flap [ď] occurs only in such pairs as riddle/little, Adam/atom, bidder/bitter, riding/writing, delighted/decided, radio/patio; it does not occur in words like cotton cited above. What this means is that it is the /t/ phoneme, in its taxonomic sense, intervocalically following a stressed vowel and standing before /ə/ or /ɨ/ plus /l/, /m/, /r/, or /d/ or before /iŋ/ or /iou/, that has changed to /d/ (at least in the dialects of many Americans). That is to say, /t/ has merged with /d/ in those positions by giving up one of its allophones in these dialects. Thus, phonemically, little is /lidəl/, atom /ædəm/, bitter /bidər/, and writing /raydiŋ/.

What should be emphasized here is that the merging has not taken place if the weakly stressed /ə/ or /ɨ/ is followed by /n/ or if /t/ is in the word-initial or -final position.[12] Consequently, heighten is still /haytən/ and tight /tayt/, just as Haydn remains /hayden/

and died /dayd/. Whether words like heighten, threaten, and sweeten are phonetically [hayʔn̩], [θrɛʔn̩], and [swi:ʔn̩], respectively, to many American speakers is immaterial here. Cf. Bloch's (1948) account of mitten ['mIʔn̩] pronounced with a glottal stop instead of [t] (PT:173).

Now, if generative phonologists like Chomsky want to treat Adam and atom in certain dialects of American English as /ædəm/ and /ætəm/, respectively, when the two words have already merged, and call the treatment systematic phonemics, even though phonetically both words are [ædəm], I see no reason why the fault should be blamed on taxonomic phonemicists. It is Chomsky and Halle and their followers who wish to hold back the changing tide of English with disguises of symbols and rules, by refusing to accept the fact that /t/ has changed to /d/ in those dialects in the positions stated above. This is a case in support of Ferguson's remark (1962) that "A synchronic analysis which ties phonology and grammar up in a neat bundle not only falsifies the current situation but makes it impossible to understand the diachrony. One area of flux which is of importance diachronically is the borderline between phonology and grammar, where the fit of the two segmentations shifts and structural changes take place" (PT:373). But note that Chomsky and Halle have disregarded the distinction of synchrony and diachrony in their phonology, though not in their syntactic component. For instance, Halle (1962) argues that in the absence of documentary or other external evidence, the simplicity criterion, which dictates to a considerable extent according to Halle a particular order of rules, is the only remaining basis for establishing the chronology (PT:389). He then cites the relative chronology of Grimm's and Verner's laws to support his claim--" a radical departure from the traditional view that synchronic and diachronic facts must not be mixed" (PT:294). But nowhere is simplicity measure claimed to be the basis for setting up the chronology in the syntactic component of their grammars. I shall have more to say in this connection later.

Returning to Adam and atom, if Chomsky and Halle wish to establish /ædəm/ and /ætəm/, respectively, as the etymons of the two words in those dialects which have collapsed them as [ædəm] phonetically or /ædəm/ phonemically, and call the establishment systematic phonemics, it is perfectly all right in what taxonomic phonemicists have called diachronic linguistics but has no place whatsoever as a level of representation in what they have properly called synchronic linguistics. I therefore suggest that it is Chomsky's systematic phonemics that should be removed from all levels of representation in synchronic linguistics, where /t/ has merged with /d/, and not taxonomic phonemics. Worse still, while taxonomic phonemics is capable of handling coexisting phenomena in terms of sound change which has affected certain words but not certain others, Chomsky's systematic phonemics is totally incapable of doing so, as Vachek (1964) has rightly pointed this out: "Chomsky's theory of grammar appears to be too static, not taking due account of the fact that language--and particularly its phonological level--must be conceived as a structure

in motion" (PT:429).

This criticism, coupled with Ferguson's cited above, may be interpreted to mean three things: One, while the syntactic component of transformational grammar is synchronic, its phonological component is a blend of synchrony and diachrony, a contradiction in theoretical construct that makes the alleged integration of phonology and grammar a sheer farce. Two, if the two components were indeed a neatly tied up bundle, a non-autonomous view Chomsky and Halle have claimed (PT:421), with the phonological component being an input-output device "which operates on a string of formatives, provided with a structural analysis by the syntactic component," and assigning to this string a representation as a string of phones (PT:401), where on earth would Chomsky and Halle generate a formative like atom with the representation /ædəm/ [ædəm] when there is no information in the syntactic component to the effect that words like atom and writer, but not button and cotton, have changed their phonemic make-up, which is essential to determining the phonetic form (PT:401) with rules like (28) and (29)? Would they posit such ridiculous morpheme features as [+original] and [-original]?[13] And, three, if a rule such as /t/ → /d/ in the environment stated above is installed in the phonological component to account for the change, isn't that the same as confirming the separation of synchrony and diachrony which must assume the autonomy of phonology (at least to the degree advocated by Vachek and Ferguson)?

2.3. Neutralization. Unlike phonetic similarity, neutralization is a concept better acquainted in Prague school than elsewhere. Basically, it is an idea to handle sounds that are not sufficiently differentiated and have uneven (i.e., defective) distributions. Both conditions, that is, insufficiency in phonetic differentiation and deficiency in distribution, must be met to qualify the notion of neutralization. Martinet himself gives an example: "The non-occurrence of initial tl in English, he says, is no ground for saying that initially before l the contrast p:t or the contrast k:t is neutralized" (Hockett 1951) (PT:313).

The result neutralization yields is the idea of archiphoneme which was first introduced by Jakobson (1929) and later abandoned by him who then preferred a treatment in terms of distinctive features (PT:293). For the sake of history of linguistics, let me repeat below some of the highlights with respect to neutralization in order to point out the absurdity and/or contradiction that has come out of the work such as Chomsky's which makes use of it. First, here are what Harris (1944) had to say.

"The phonologists of the Prague Circle tried to indicate some of these limitations of distribution by saying that a phoneme which does not occur in a given position is 'neutralized' by one which does, and that an 'archiphoneme' symbol can be written to represent either phoneme in that position. Thus /b/ and /p/ are neutralized after /s/, and can then be represented by the archiphoneme P, which would indicate

the 'common element' of both: /sPin/ instead of /spin/. This did not in itself prove to be a productive method of description. In the first place, most cases of 'neutralization' involve not merely two phonemes that directly neutralize each other. Usually several phonemes occur in a given position while several others do not, and 'neutralization' may be said to exist between the two whole classes of phonemes; thus after word-initial /s/ we find /p, t, k, f, l, w, y, m, n/ and the vowels, but not /b, d, g, v, θ, ð, š, ž, s, z, r, ŋ, h/. To select /p/ and /b/ out of the two lists and assign them to a separate archiphoneme P implies some further and hitherto unformulated method of phonemic classification on phonetic grounds. And what shall we do with /θ/ or /s/ or /z/?"(PT:116-7).

Second, in (1941) Harris also pointed out the following. "Two phonemes may be contrasted in some positions but not in others, if in these other positions only one of them can occur. For example, English [b] and [p] are not contrasted after [s], because only one of them can occur after [s]. Neutralization (Aufhebung) is the term for such lack of contrast in specific positions... Like other European linguists, he (i.e., Trubetzkoy--FCCP) discusses whether the phoneme in neutralized position should be regarded as representing one of the neutralized phonemes or both together..." (PT:304).

Similarly, Hockett (1951) had the following to say, "Unlike Bloomfieldian Phonemics, Trubetzkoyan phonology does not stop when phonemes... have been determined. Analysis continues... Trubetzkoyan workers then take another step. In French and in Russian, for example, there occur pairs of phonemes, such that the members of each pair share all distinctive features save one, and differ only in that one: thus, p : b, t : d, and so on, the distinguishing feature here being the voicing of the second member of the pair. Each such pair constitutes an archiphoneme. In French, there are no positions (or no important ones) in which p is found but b is not, or conversely; and similarly for the members of each other voiceless-voiced archiphoneme. In Russian, on the other hand, though both voiced and voiceless members occur initially and medically, only a single type is found finally. The difference between voiced and voiceless makes a difference initially and medially, given the proper assortment of other features in a bundle; finally however, that difference is not functional. Thus byl' (a fact) and pyl' (dust) are different words; zabivat' (to thrust) and zapivat' (to start drinking) are different words; but corresponding to [pɔp] (priest) there is no *[pɔb], nor is any other such pair to be found.[14] The Trubetzkoyan way of stating this situation is to say that in final position the opposition--which is simply a synonym of the American term 'contrast'--between voiced and voiceless is 'neutralized', and that the [p] that one hears finally, despite its objective similarity to initial and medial [p], is not to be interpreted as phonemically the same, but rather as an occurrence of the p/b archiphoneme. The term 'realize' comes in here too: in Russian, the p and b phonemes are 'realized' in certain positions, whereas in another only the p/b archiphoneme is 'realized'; in French, there is no position in which the p/b archiphoneme is 'realized' as such, even though it exists

there just as much as in Russian" (PT:312-3). To this Hockett added a footnote by Martinet as follows: "But as this archiphoneme is, in that language, never realized as such, it is probably just as well not to mention it" (PT:313).

Hockett continued. "The traditional American way of stating the same situation is at least superficially different: in Russian, the phonemes p and b both occur initially and medially, but finally only the phoneme p occurs; that is, voiceless phonemes, of those that are paired with voiced phonemes, occur in final position to the exclusion of the latter" (PT:313).

Besides two usual American objections, Hockett raised a different objective. "It may be suspected that 'neutralization' is too simple a notion to handle all the relevant types of distributional phenomena which deserve mention in a phonological description... This means that the notion of neutralization can handle only some of the distributional limitations in a language. There is a class of facts which can be subsumed under 'neutralization'; this class includes the non-occurrence of final voiced stops in Russian but not the non-occurrence of initial tl in English" (PT:313).

Returning now to the examples of rider/writer and the like, since we have already considered two solutions[15] for certain groups of American speakers, that is, [d̆] and [t̆] for respective pair-words or [d̆] for both members of each pair, we can proceed with a third possibility. This solution, however, does not have a constituency, as it were, of its own in the sense of having separate groups of native speakers; it is simply a different way of stating the same facts which underlie the second solution. The third solution thus follows the definition of neutralization presented above. That is to say, the phoneme /d/ and the phoneme /t/ are contrasted in initial and final positions but not in medial position, save for pairs like sudden/button. In such a position, only the d/t archiphoneme is realized, which is to say that the opposition between voiced and voiceless is neutralized in that position, and that the realization of the d/t archiphoneme one hears medially, despite its objective similarity to initial and final [d], is not to be interpreted as phonemically the same, but rather as an occurrence of the d/t archiphoneme. Note the phrases objective similarity and not to be interpreted as phonemically the same.

But when we turn to Chomsky for the use of archiphoneme, we find a different version, one that is treated at the phonetic level. In (1957), he said "Consider, for example, the case of an English dialect where medial post-stress /t/ and /d/ merge in a tongue flap [D] and where only the normally redundant feature of vowel length distinguishes writer (phonetically, [rayD±r]) from rider (phonetically, [ra·yD±r])" (PT:347). In (1964), he said "the notation $[s_1,s_2]$ is used for the 'archiphoneme' consisting of the features[2] common to s_1,s_2" (PT:404).

Recall that his rule (29) is a phonetic rule, giving rise to his systematic phonetic representation (PT:405). More deviated, however, is the fact that Chomsky, as will be shown later on (cf.§4.2.2), even used his "archiphoneme" to be something that can be either voiced or voiceless as he commands.

The above comparison clearly suggests that Chomsky's version of archiphoneme or neutralization, in one way or the other, is not warranted. Because, if it were, the pair-word would have to be represented phonemically (or rather archiphonemically) as ray[t,d]ɨr and ray[t,d]ɨr or rayDɨr and rayDɨr, respectively. At this level of representation, where would Chomsky's rule (28) apply? It simply falls flat, because its applicability depends entirely on the voicing of the second word against the voicelessness of the first word, an opposition no longer in existence as it has already been neutralized phonemically (or archiphonemically) as [t,d] or D in medial position.

Earlier, I have cited some examples in Mandarin (cf.§ 1) where neutralization of a seemingly different sort was mentioned. This practice is in line with Y. R. Chao (1957 & 1968) as far as Mandarin tonal neutralization is concerned. Without going into the detail of the taxonomic phonemic analysis of Mandarin tones, let me quickly point out that when Mandarin tones are neutralized, they become atonic (archiphonemically), there being four phonemic tones. But I must add that this archiphonemic (or neutralized) tone is not represented with a symbol, although one could do so by assigning 5 to it. It follows that really there isn't any difference, the only superficial deviation being that instead of two sounds four tones are neutralized in a certain position.

Now, if Chomsky wishes to conform to such a practice in neutralization, which has been useful in the tonal analysis of Mandarin, rider/writer would have to be represented as ray()ɨr/ray()ɨr where the parentheses contain no symbol. His rules would definitely go haywire, if the archiphoneme were not represented.

Chomsky's rule (29), however, may still apply, if modified, to the degree limited by the counter-examples provided further above. That is, the rule will merely show the realization of the archiphoneme [t,d] in certain contexts for words like writer and little for those to whom it applies. But this realization is not [D], as Chomsky has asserted it to be for (29). Rather, it must be modified as [ď] (voiced, alveolar flap).

A related problem is the difference in the length of the stressed vowel in rider/writer. This problem is solvable when and only when the following consonant is voiced for rider but voiceless for writer. But since Chomsky has claimed the use of the archiphoneme [t,d], which is phonetically [D] to him, it is neither voiced nor voiceless. Consequently, he faces another dilemma of which he is not even aware.

That is, he could salvage rule (29) by clinging to the archiphoneme [t,d], i.e., the third solution, thereby starting with ray[t,d]ɨr/ray[t,d]ɨr, instead of raydɨr/raytɨr, and giving up rule (28); or, he could salvage rule (28) by clinging to the d/t phonemic distinction, i.e., the first solution, thereby insisting on raydɨr/raytɨr, instead of the archiphoneme [t,d], and sacrificing rule (29). The two rules are not salvageable, however, if Chomsky is forced to face the second solution; because t as a phoneme is already replaced by d as a phoneme in that position.

Before I go on to the next topic, let me point out one more fallacy of Chomsky's rules. Recall that he has repeatedly said that the distinction of pair-words like rider/writer depends on normally redundant feature of vowel length (PT:347 & 410).[16] Since no one has ever produced any empirical evidence to attest this claim, such as those presented by Haugen and Twaddell (1942), the distinction deserves some careful discussion here.

First, consider once more what Sapir has said concerning bad/bat, etc. We do have data to attest that the vowel before d is longer than, though not quite twice as long as, that before t in the pair. For example, 0.32/0.21 sec. for Heffner and 0.29/0.25 sec. for Lehmann for the vowel [æ] before d/t according to Haugen and Twaddell (PT:93). We even have the length of [ɑi] before d/t for the two speakers; namely, 0.32/0.24 sec. and 0.32/0.23 sec., respectively, corresponding presumably to pair-words like ride/write (PT:93).

But, second, note that the alternations are governed by mechanical considerations, as Sapir has already noticed, a point that may be inferred to mean that if the conditioning factor (i.e., the d/t distinction) is eliminated, the alternations do not exist. I may add that this is what complementary distribution means in taxonomic phonemics.

Now, if dissyllabic pairs like rider/writer, instead of monosyllabic pairs like ride/write and bad/bat, are examined, I believe the inference still holds, unless counter-evidence is produced to the contrary. That is to say, the alternations are governed by mechanical considerations in rider/writer, provided the d/t distinction is maintained but the alternations are no longer there once the d/t distinction ceases to exist.

In the first solution, where the flaps are [ď] and [ť], it may still be said that the vowel before [d] is longer than that before [t] but I am not so sure that if native speakers fluctuate between [ď] and [t], the vowels follow suit. This is precisely what has happened to Johns (1969) whose spider and cider rhyme not with rider but with writer but who gets [spəy-dr] and [səy-dr], when he over-enunciates these words; that is, since he keeps rider/writer separate, because of rhyming, when he rhymes spider and cider with writer, he uses the vowel of writer for spider and cider as it is governed

by the consonant, yet when he returns to [d] for spider and cider the vowel of spider and cider does not match that of rider, as it should, but remains the same as that of writer.

I am willing to go one step further to say that Chomsky's assertion that the normally redundant feature of vowel length distinguishes pair-words like rider/writer is false. In the case of Johns (1969), for instance, it is vowel quality, i.e., different vowel phonemes, /ə/ and /a/, that separate rider [ra:yDr] from writer [rəyDr] (PT:550). Consequently, whether the medial consonant [D] is interpreted as [ď] or [ť], respectively, or [ď] for both words is totally irrelevant in his dialect, the length in [a:] being not functional. In the event that the flap is [ď] in both words, be it treated as the phoneme /d/ (i.e., the second solution) or the archiphoneme [t,d] or /D/ (i.e., the third solution), I dare say that if the vowel qualities are identical, for speakers like Chomsky, the difference in vowel length is nill, unless Chomsky and Halle can produce convincing data to the contrary like those provided by Haugen and Twaddell.

3. Relations and Patterning. "Few languages," said Bloch (1948), "are so simple that their phonemic system can be economically described without invoking at least some of the criteria that are currently grouped under this heading" (PT:195). Important as relations and patterning may be, sometimes disputes arise when differing weights are placed on them. Examples are Trager and Bloch (1941) and the responses from Haugen and Twaddell (1942), Pike (1947b), and Kurath (1957) concerning long vowels, diphthongs, and initial and post-vocalic /h/, among others.

Certain focuses in one school of thought have also been misinterpreted by members of another, because of relations and patterning. A good example is Robins (1957) who commented on Pike's Phonemics (1947a) by saying that "phoneme theories have necessarily concentrated on minimal contrast in identical environment, emphasizing the paradigmatic aspect of phonological relationships at the expense of the syntagmatic or structural aspect..."(PT:266), when in fact "in America the main emphasis was placed on the distribution of phonemes and their allophones" (PT:293). It was Prague school linguists who "were more interested in stating the phonetic contrasts among the phonemes, that is, in noting those features that serve to distinguish one phoeneme from another" (PT:293).

Too much interest in relations and patterning has, on the other hand, let Chomsky and Halle to make such blundering statements as systematic gaps versus accidental gaps (1965). Note that Swadesh had used the phrase "systematic gaps" thirty years earlier to mean something quite different: "It is well to insist on the essential nature of systematic gaps of distribution (e.g., that a form like *seeng does not exist and would not fit into the English phonemic pattern)"(PT:44). Chomsky and Halle's blunder has been pointed out in Peng (1969 & 1972a). Kučera (1967) has also, independently, called Chomsky and Halle's bluff on their account of /blik/ and /bnik/ (PT:491).

A different kind of blunder has also been committed by Chomsky regarding the palatalization of k and t (PT:404) and of k, g, and t (PT: 545-6). Vachek has already responded to this, with many a counter-example, by pointing out that Chomsky's rule "has not a general validity: it can only be applied to the lexical layer of synchronically foreign English words (taken mostly from French or Latin), while in the synchronically domestic words, forming the core of ModE vocabulary, it does not assert itself at all"(PT:424-5). But I must hasten to add that even in words of Romance origin, Chomsky's rule does not apply, as may be evidenced by the pair patio/ratio or by such individual words as tutti, tutti-frutti, and tutty. Also of interest is the famous fashion model's name Twiggy which is [twigi] not [twiǰi], as Chomsky's rule predicts, although it definitely comes from twig because of the model's skinny figure. Cf. Peggy, Maggie, piggy, and, of course, doggie, dogginess, doggish, doggy. Words such as these far out-number Chomsky's rigid/rigor and legislate/legal as to make his rule (PT:532)

$$(9)\quad \left\{\begin{matrix} k \longrightarrow s \\ g \longrightarrow \check{j} \end{matrix}\right\} \Big/ \begin{bmatrix} \_\_\_\_\_ \\ -\text{grave} \\ -\text{compact} \end{bmatrix}$$

look more than ridiculous.

4. Status of the Phoneme. Having sorted out some of the serious problems that exist in generative phonology, from the point of view of history, I shall now switch to a different angle--that of generative phonologists' view of taxonomic phonology--to see if their criticisms, which are all but taken for granted and accepted as the eternal truth by a new generation of young linguists, have been justified and fair. The reason is that most of these young linguists have been given only one side of the story--the highly biased side--and never had a chance to really appreciate the achievements produced by non-generative phonologists. It is, therefore, high time that I bring out some of the hidden facts, not to defend the phoneme nor to revive it (because it is not dead), but to straighten out the record, as it were, for the younger generations.

This being the case, I shall not go into the detail of every facade of the discussions, for or against the phoneme, directly or indirectly. Rather, I shall be concerned with two specific aspects of the status of the phoneme: one, the status of the phoneme as attacked by generative phonologists, Chomsky and Halle in particular, which will be referred to as +Attack; and, two, the status of the phoneme as exploited by generative phonologists, explicitly or implicitly, which I speak of as -Attack.

4.1. +Attack. The first attempt to discredit the phoneme, in the hope to eliminate its status as a level of representation (cf. §1), appeared in Halle (1958) which was later revised as Halle (1964).

Such an endeavor, in my opinion, was carried only to the extent of a notational modification and, therefore, has had no bearing whatsoever on the elimination of the phoneme itself, contrary to the widely held opinion among all walks of linguists that the phoneme has been eliminated because Halle showed how morphemes in a generative grammar could be represented directly by distinctive features. Two reasons may be stated below.

One, in his review of Jakobson et al. (1952) which was later revised as Jakobson and Halle (1956) which in turn became the foundation of Halle's assault, Chao pointed out that "The idea of distinctive features and of bundles of them forming phonemes were the very things--the very words--used by Bloomfield (1933:79), who left to future investigators to find out what they really were, ..."(1954:41). He also said that Jakobson et al. "are among Bloomfield's future investigators" (1954:41). The only difference is the added assertion made by Jakobson and company for the first time that there is a limited number of distinctive features which can be used to describe all the languages of the world. But this obviously over-stretched claim has since become one of the major arguments given for the superiority of the distinctive feature approach. Controversy aside, it must nevertheless follow that the fact that now morphemes are represented directly by distinctive features (Jakobsonian or otherwise) instead of phonemes (i.e., bundles of distinctive features) is totally irrelevant to the question of whether or not the phoneme has been eliminated.

Perhaps, an analogy would illustrate this point more clearly. Physicists speak of molecules being made up of atoms. But they also speak of an atom being made up of even smaller particles, such as the nucleus. Is there, then, any ground on which to argue for the elimination of the atom? If physicists would not throw away (or could not, even if they dare) the status of the atom, just because they have now discovered the nucleus consisting of several different kinds of particles, packed closely together, why should or how could linguists afford to abolish the status of the phoneme, just because morphemes can now be represented directly by distinctive features!

Two, for the sake of the history of linguistics, I must add that the original intent of Jakobson et al. (1952) was not the elimination of the phoneme. Rather, it was to help solve some of the problems in phonemic analysis indicated by Chao (1934), as may be evidenced by the following passage: "By successively eliminating all redundant data (which do not convey new information) the analysis of language into distinctive features overcomes the 'non-uniqueness of phonemic solution.' This pluralism, pointed out by R. Y. Chao, interferes with the analysis as long as the phoneme remained the ultimate operation unit and was not broken down into its constituents. The present approach establishes a criterion of the simplicity of a given solution, for when two solutions differ, one of them is usually less concise than the other by retaining more redundancy" (1952:7). But note that despite the "new" approach similar, if not worse, pluralism

continues to exist in distinctive feature analysis (Jakobsonian or otherwise) as well as in generative phonology which makes use of it (cf. Peng 1972b:272).

More fearful attacks, however, have come from Chomsky (1964) whose ostensible "evidence" against the phoneme has scared many American taxonomic phonemicists away, some even from their jobs, for the past decade or so. Below, I shall prove that the phoneme has not been eliminated and that the various attempts to do so do not hold any water, albeit successful in painting a new front.

One such attempt is Chomsky's /sKKt/ [sǎkt] 'socked' and /sKKt/ [skǎt] 'Scot' (PT:415). He took great pains to argue that [k] and [ǎ] are in complementary distribution in English (and, furthermore, share features shared by nothing else, e.g., in Jakobson's terms, the features Compact, Grave, Lax, Non-Flat). Hence they qualify as a tentative phoneme, and there is a tentative phonemic system in which they are identified as members of the same phoneme /K/ (PT:415). That a vowel like [ǎ] is found to be in complementary distribution to a consonant like [k] is not at all Chomsky's discovery. Thirty years earlier, Swadesh, a great American phonemicist and pioneer, had already said that "As a matter of fact, any vowel would be found to be in complementary distribution to almost any consonant" (PT:36). So, American phonemicists in general had been aware of this peculiarity, an intriguing fact in human (vocal) language which is discoverable only through the application of a rigorous concern of distribution. Such is not the case in Chinese and Japanese, however, e.g., in Amoy [ŋ] 'yellow' contrasts with [o] 'lake', [e] 'shoe', and [i] 'aunt' (all with the same tone which is omitted) and in Japanese [kan] 'liver' contrasts with [kao] 'face'. Cf. English attack/stack.

What is wrong with Chomsky's example then? I can only say, with respect and honesty, that it is Jakobson's poor features and Chomsky's blindness in adhering to those features that must bear the responsibility of the error and not the status of the phoneme. Why? Because, within the framework of generative phonology, using good and solid features like High, Low, and Back (Schane 1973:31 & 35) [k] and [ǎ] are distinctive to begin with; therefore, a straightforward phonemic result, /sakt/ and /skat/, is borne out. Notice carefully that the features Schane uses are articulatory features, the same kind of features Bloomfield had conceived of, which had been employed by taxonomic phonemicists for decades and that the features Chomsky relied on were blends of acoustic distinctive features and the characterization of each in terms of the shape and size of the resonators involved, but with the insistence that the acoustic statements are linguistically more relevant (PT:294). We can see very vividly that young generative phonologists like Schane have abandoned Jakobsonian features and adopted back the traditional phonemicists' (articulatory) distinctive features.

In the same (erroneous) vein, Chomsky argued that "[ə] and [r]

are in complementary distribution (and share defining features) and thus qualify as a potential phoneme" (PT:415). This time he uses /pRRvěl/ [prəveyl] 'prevail' and /pRRvěd/ [pərveyd] 'pervade' as a case against local determinacy and biuniqueness. But note that nowhere has Chomsky ever said what he means by "defining features" which is nothing more than a paraphrase of phonetic similarity. Here, I cannot help but say that Chomsky's judgment of phonetic similarity is much more "dangerously loose" than Haugen and Twaddell would wish to have described him. For Chomsky has obviously mistaken [ə] for [ɚ] which is a special r-vowel (PT:94). It is, I believe, this kind of "goof-off"--an act that amounts to no less than an intentional distortion of facts--that led Householder (1965) to respond that "Many of Chomsky's arguments imply the existence of some solid basis for phonetic identity which is unknown to me" (PT:452). If there is resemblance, it exists between [ɚ] and [r], not between [ə] and [r]. I may add that [ɚ] also resembles [ə],[17] a fact that has already been succinctly stated by Bloch (1948) in terms of features: "The retroflex vowel [ɚ] of American English contains two features: raising of the surface of the tongue ... and raising of the tongue tip. The segment [ə] of sofa contains the first of these features alone" (PT:194). In line with Bloch, then, the segment [r] would contain the second of those features, thereby making [r] and [ə] dissimilar.

As a result of the above added information, pervade would be phonetically [pə˞veyd], yielding phonemically either /pərveyd/ (PT:87) as Trager and Bloch might have done or /pə˞veyd/ (PT:94) as Haugen and Twaddell might do, depending on whether [ɚ] is treated as a "suppletion" of two phones or simply one special phone.[18] In the latter case, since [ɚ] and [r] are phonetically similar and in complementary distribution, an alternative would be /prveyd/ (cf. [səy-dr] 'cider' in Johns's pronunciation). But no such alternatives are feasible in the case of prevail.[19] A different pronunciation from Chomsky's is [priveyl] which means that the word is either /prəveyl/ or /priveyl/ phonemically. So, once again, it is Chomsky's deliberate distortion of phonetic facts, not the status of the phoneme based on the facts, that is in error and violates local determinacy and biuniqueness.

To argue against the status of the phoneme still further, Chomsky invoked phonemic overlapping which he said illustrates the violation of his invariance condition while preserving linearity. What he failed to mention is that phonemic overlapping, first suggested by Bloch (1941), has been abandoned since, due to insufficient differentiation of the sounds in question, by taxonomic phonemicists (perhaps Bloch himself as well). This being the case, let us examine Chomsky's argument to see if it is valid. Here is what he said:

"Thus consider an English dialect in which [D] is the allophone of /r/ in 'throw' and of /t/ in 'Betty' (where it contrasts with the /r/ of 'berry' ...). Following the principle of invariance, we must assign [D] to /t/ in the context #θ__, counter not only to the speaker's

intuition but also to the otherwise valid rules of consonant distribution. The situation is worse in dialects in which [D] and [r] are in free variation in this context and in intervocalic contrast, in which case no coherent solution is possible ..." (PT:412).

Notice that the notorious [D] has appeared again. Since he has never said whether it is voiced or voiceless, let me go back to Bloch (1941) where at least I can find some phonetic value. "The flap after [θ] is often partly or wholly voiceless; but the voiced variety also occurs" (PT:67). With this much phonetic information, if I follow Smalley closely I come up with [R̥] and [r̥] for the voiceless and voiced varieties, respectively. Moreover, Bloch also said that "In the speech of many Americans, the [t] phoneme includes as one of its constituent sounds or allophones an alveolar flap (something like the r of London English very), which occurs intervocalically after a stressed vowel, and in this position varies freely with the familiar voiced t and with the aspirated voiceless t, as in butter, ..." (PT:67). Here, in keeping with Smalley's notation, I get [ť] for "something like (which is not the same as) the r of London English very," [ď] for "the familiar voiced t," and [th] for "the aspirated voiceless t," the r of London English very being transcribed as [ř] by Smalley.

Given this kind of data, who would assign [ť] or [ď] to /t/ in the context of #θ__? The logical solution is simply to assign [ť] and [th] to /t/ for words like Betty, [ď] to /d/ for words like kiddy (or kitty for those who have merged /t/ with /d/), and [R̥] and [ř] to /r/ for words like throw, berry, or very. So, once again, it is Chomsky's deliberate disguise of symbols, poor differentiation of sounds, and intentional cover-up of phonetic facts that have led to his chaotic confusion. The burden falls on him, not taxonomic phonemicists. Other similar examples raised by Chomsky against the status of the phoneme are equally fallacious and, therefore, need not concern us here further.

4.2. -Attack. Having shown that generative phonologists' attack on the phoneme is without foundation, I shall now address myself to the other side of the same coin to see if they have honestly stayed away from the phoneme. They have, I think, in two respects: one, in tones and intonations, as was mentioned earlier (cf. §1); and, two, in segmental phonology. Tones and intonations aside, because they have not been able to find a convenient substitute, segmental phonology requires some explaining, because they have found a substitute called systematic phoneme which is highly tricky and sometimes deceptively dubbed as the morphophoneme. So, let us examine closely the literature for a clean-cut appraisal.

In 1971, Schane published an interesting article entitled "The Phoneme Revisited" in which he has an insightful remark: "Since there are so few 'low-level' phonetic rules, it follows that the output of any of these generative phonologies must be a surface representation quite similar to a classical phonemic one. One has only to look at

various generative descriptions to see that this is indeed the case. A good example can be found in Chomsky & Halle's chapter IV, dealing with the segmental phonology. The rules given there are all morphophonemic in function; but every surface form resulting from derivations is equivalent to a Trager-Smith phonemic representation" (1971: 519-20).

More interesting, however, is his tongue-in-cheek conclusion: "The phoneme was the offspring of structuralism, the pride and joy of post-Bloomfieldian linguistics. Since then the child has been abandoned. Yet some of us may have felt guilty about disinheriting the child. As generativists, if we acknowledged him, then it was as an illegitimate child. Perhaps we can now recognize the little bastard for what he really is" (1971:520).

One particular point will merit some further discussion here; that is, although generative phonologists like Schane have admitted that the phoneme has been very useful in generative phonology, he has limited its usefulness to the surface representation, simply because there are so few 'low-level' phonetic rules. But I am of opinion that their systematic phonemic (often referred to as underlying) representation is <u>not</u> devoid of the phoneme either. Let me now cite another example from Chomsky (1964).

(22)	lajik + yɨn	prezident + i	prezident + i + æl
	lajis + yɨn	prezidens + i	prezidens + i + æl
	lajisɨn		prezidenš + æl

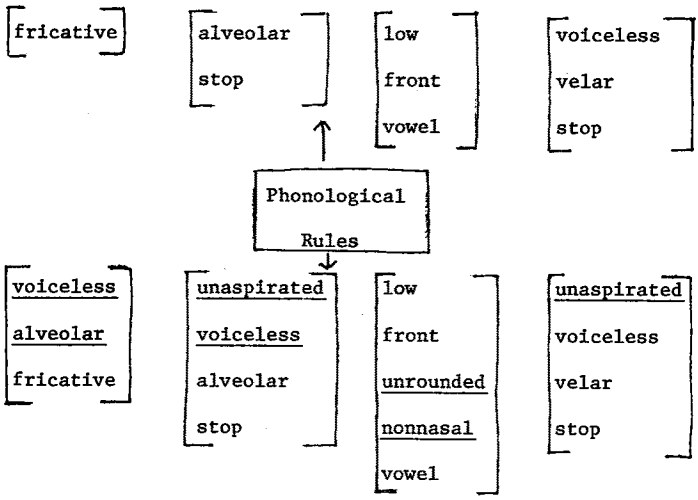
According to Chomsky, the top line in (22) is the systematic phonemic representation, in each case, and the last line becomes the systematic phonetic by additional rules (<u>PT</u>:404). But observe that the only difference between the top line and the bottom line in each case is the k/š alternation for the first pair (<u>logic</u>/<u>logician</u>), the t/s alternation for the second pair (<u>president</u>/<u>presidency</u>), and the t/š alternation for the third pair (<u>president</u>/<u>presidential</u>), the rest in both lines being completely identical. Here, a closer look at Chomsky's systematic phonemic and systematic phonetic representations is necessary. So, I will divide the argument into two parts.

4.2.1. <u>Underlying versus Surface Representations</u>. First, my argument has to do with the fact that except for the morphophonemic alternations (in the traditional sense of the term) the rest of each pair in both lines are exactly the same. This gives rise to the logical corollary that if the last line is the systematic phonetic (or surface) representation which is in turn equivalent to a Trager-Smith phonemic representation, resulting from derivations by the application of rules, then it must follow that the so-called systematic phonemic (or underlying) representation must be none other than the same Trager-Smith phonemic representation except for those segments engaged in morphophonemic changes.

This being the case, there is actually no distinction between the underlying and the surface representations in generative phonology, save for those segments that undergo morphophonemic changes. In other words, if no morphophonemics is involved, the underlying representation is equal to the surface representation which is in turn equal to the Trager-Smith phonemic representation, unless some low-level phonetic rules are applied. For instance, the underlying forms of logic and president are identical with their surface forms, both representations being lajik and prezident, respectively, unless some phonetic rules are applied to specify that the final -k and -t are unaspirated. If such is the case, it is just another way of indicating narrow phonetic facts.

To overcome superficial distinction of this kind, generative phonologists did try hard to find a more substantial distinction between the two levels of representation. Another example, from Langacker (1973:161), will suffice to illustrate this point.

UNDERLYING PHONOLOGICAL REPRESENTATION



PHONETIC MANIFESTATION

This example shows how one would derive the phonetic manifestation from the underlying phonological representation by way of certain low-level (phonological, i.e., phonetic) rules, the underscored features being the added results. Notice that there are exactly four segments in each representation corresponding to the word stack. But observe that in taxonomic phonemics the same word is represented phonemically as /stæk/ and phonetically as [stæk], exactly four segments in each representation. Taxonomic phonemicists can then describe each such segment in terms of its articulatory features, with varying degrees of detail, distinctively in /stæk/ but non-distinctively in [stæk], corresponding exactly to Langacker's feature-specifications.

What I have shown here is that when lexical items, such as stack and stag, involving no morphophonemic alternations, are specified in terms of features, the so-called underlying phonological representations are on a par with taxonomic phonemic representations, one-to-one, no more and no less, but the systematic phonetic representations may or may not correspond to taxonomic phonetic representations. If low-level phonetic rules are available and applied, the systematic phonetic representations are equivalent to narrow phonetic transcriptions (in the sense of taxonomic phonetics); if no such rules are available, they are equivalent to broad phonetic transcriptions (which are often identical with taxonomic phonemic representations).

It must be granted that innovations regarding feature specifications in generative phonology have been made. But I must quickly add that they are very few and very minor. A good example is the segment s in the underlying representation. Normally, the voiceless and alveolar features are distinctive for s. At the beginning of a morpheme before a stop, however, they are not distinctive; hence, only [fricative] is assigned to it. But whether s is regarded as consisting of only [fricative] to be distinctive in stack or of [voiceless, alveolar, fricative] to be distinctive in see is immaterial, as s continues to be a bundle of distinctive feature(s) in its Bloomfieldian sense. If generative phonologists wish to call it a systematic phoneme, it is perfectly all right. The important fact, however, is that the phonemic status of s remains unchanged, even though a different label is attached to it.

4.2.2. Morphophonemics. The second part of my argument has to do with the k/š, t/s, and t/š alternations mentioned above. So, let me return to Chomsky's example (22). To handle these alternations, among others such as k/s, Chomsky set up two rules (PT:404).

(20) (i) $\left\{\begin{matrix} k \\ \\ t \end{matrix}\right\} \longrightarrow$ s in the context: __ + [i,y]

(ii) [s,z] + [i,y] $\longrightarrow$ [š,ž] in the context: __ Vowel

The second rule is then generalized to apply to [t,d] instead of simply /t/ (PT:405). The generalization is supplemented by another rule which is augmented in a footnote (PT:405) as

(23) [+Continuant] [-Voiced] in the context: __ + iv.

These rules have a serious theoretical defect that has never been pointed out before. For the sake of history, I will sidetrack for a moment to discuss them, and then return to the main argument.

Note that four Chomskyan archiphonemes are used here, [t,d] among them which has already been thrown out as no good (cf. § 2.3). I must say now that the others are just as bad, if not worse. The reason is simple. Recall Chomsky's own definition of the archiphoneme stated earlier. In keeping with this definition, the only features common to s and z in [s,z], for instance, are tip-alveolar and fricative, the voiceless/voiced distinction being neutralized. Consequently, each one of [s,z], [š,ž], and [t,d] should be neither voiceless nor voiced; he was faithful to this particular point when handling [t,d] in rule (29) in order to get his notorious [D]. But here a hitherto unheard of usage of the archiphoneme appears; every consonantal archiphoneme Chomsky uses, including [t,d], becomes either conveniently voiceless or conveniently voiced. For instance, when applied to (22), (20ii) comes out voiceless, but when applied to persuade/persuasion, it comes out voiced, as may be evidenced by the derivation

(26) perswēd + yɨn, perswēz + yɨn (by (20i)), perswēžɨn (by (20ii)).

The oddity of Chomsky's rules does not stop here. The flip-flop convenience of the Chomskyan archiphoneme suddenly disappears, when it comes to the derivation of persuade/persuasive. One would expect that if (20i) were generalized to apply to [t,d] instead of simply /t/. as Chomsky has suggested, it would certainly become something like

(20i) $\left\{\begin{matrix} k \\ \\ [t,d] \end{matrix}\right\} \longrightarrow$ [s,z] in the context: __ + [i,y]

although Chomsky himself has never stated this augmented rule. But clearly it is because of this augmentation that he has modified z$\longrightarrow$

s in the context: __ + iv as (23). However, oddly enough, when persuade/persuasive are derived, he uses the generalized (20i) first, where he still adheres to the flip-flop convenience so that [t,d] comes out voiced as z, then switches to (23) to change z into s, as may be evidenced by

(25) perswēd + iv, perswēz + iv (by (20i)), perswēsiv (by (23)).

If Chomsky's archiphoneme is so convenient that it can be voiceless or voiced at the touch of his magic wand, I wonder why he should even be bothered with (23). All he needed to do is to give a stronger command by saying "Open sesame!" thereby changing d directly to s by (20i), bypassing the intermediate step in the course of deriving (25).

Now back to the main argument! To start with, we must know what morphophonemics is like in the Bloomfieldian sense of the word, because generative phonologists have often claimed that their phonology is Bloomfieldian morphophonemics.

In 1939, Bloomfield published his well-known "Menomini Morphophonemics" according to which final n is replaced in alternation by s before e, ē, y (PT:61). Examples are: ɛn-yā-~ ɛsyā- : esyāt 'if he goes thither' and -ēn-e- ~ -ēse- : wēs 'his head' and ōn-e- ~ ōse- : ōs 'canoe'. Here, the symbol ~ means "is replaced in alternation by"; the colon (:) means "appearing in the actual Menomini word."

If the conditions so stated are not met, there is no alternation as such, for instance, ɛn-ohnɛ̄- : enōhnɛt 'if he walks thither or thus'. But Bloomfield also said that "Certain n's are not subject to this alternation; we designate these in our basic forms by N; in actual speech, then, this theoretical N is replaced by n" (PT:61). Examples: pōN-e-m- : pōnemɛw 'he stops talking to him' which contrasts with pōNɛ̄Nem- : pōnɛ̄nemɛw 'he stops thinking of him' (PT:60).

What this means is that there are two morphophonemes n and N. The first results in either s (under certain conditions) or n (if the conditions are not met) whereas the second is replaced by n throughout. The resultant n's and s are, of course, phonemes in the taxonomic sense of the word.

But when we turn to Chomsky's (22), something important is missing: He does not spell out that if the conditions which change k or t into s or š are not met, k remains as k and t remains as t, as in stack. Worse still, he is not even honest enough to admit that in English there are certain k's and t's that are not subject to such alternations, as in rock/rocky and fat/fatty (PT:424). See Vachek (1964) for the details.

Had Chomsky followed Bloomfieldian morphophonemics, on the other hand, he would have had to come up with a morphophoneme k which becomes s or š under certain conditions, as in opaque/opacity and logic/logician,

but remains as k if the conditions are not met; at the same time, he would have had to set up another morphophoneme K which is replaced by k throughout, because it is not subject to the said alternations. Similarly, he would invariably have had to recognize a morphophoneme t, having s, š, and t as its members, as in presidency/presidential/president depending upon the conditions, plus another morphophoneme T which remains as t because of its independence from the alternations. The resultant k, t, s, and š are phonemes.

Why didn't Chomsky do all this to begin with? This is an intriguing question to ask and, therefore, will merit some more discussion. A quick answer would be to say that he does not know morphophonemics or is not aware of morphophonemic irregularities. I am inclined to dismiss this kind of easy answer, because it is not constructive and will not benefit the academic community in linguistics. After all, both Chomsky and Halle were brought up by great structural linguists. So, they could not have missed the instruction on morphophonemics. In fairness to Chomsky, we must look elsewhere for a justification of his errors.

The main reason, I think, is his intent to avoid the status of the phoneme. With the attack described earlier (cf. +Attack) in mind he could not possibly have conformed to morphophonemics, because to account for morphophonemic alternations in any language entails having the phoneme as its operation unit. So, what Chomsky has done, then, is to take one of the resultant phonemes (which he calls a systematic phoneme, say, k) and derive from it another one of the phonemes, say, š, but this time put š in his systematic phonetics. In the process of such a derivation (his application of the rules mentioned above) he conveniently (1) loses the vital information that if the condition is not right k comes out as k in the output and (2) forgets that there are phonemes (or morphophonemes in the sense of K and T) that are not subject to his flip-flop rules.

Judging from the preceding discussion, it is absolutely clear that systematic phonemics is not morphophonemics but something else. Only occasionally does systematic phonemics handle morphophonemes, a case in point being the Russian example illustrated earlier. There, the palatalized t (i.e., t,) in Column I is an authentic morphophoneme as I have already pointed out. But Chomsky stopped short of deriving t, and d, in all positions from the morphophoneme; that is, not only in Column III but in initial position as well. I seriously challenge Chomsky to do so without the phoneme.

5. Conclusion. There is much more that can be said concerning generative phonology, such as how abstract or concrete generative phonology is supposed to be. But we can forego this for the time being, as I have presented enough evidence on the basis of which to pronounce a verdict. Before I do so, however, we must answer this question: What have we learned as a lesson from half a century of development in phonology?

First, we have learned, at least I have, that it was wrong for American structuralists (taxonomic phonemicists in particular) to have assumed a single entity in their handling of the phonological system of a language. There were, of course, exceptions who would allow for coexistent variations. By and large, however, structural diversities were more often than not left to dialectologists and historical linguists. This particular flaw was brought forth, constructively, I think, by Lyons (1962) more than ten years ago: "The 'orthodox' American view seems to be: (a) that the phonology of any given language is a uniform system; and (b) that phonological analysis can, and should, be carried out independently of grammatical analysis. Neither of these propositions is acceptable" (PT:280).

While I am inclined to accept (a) as a fair statement, even to the extent of concurring with him that "the phonology of a language cannot be described satisfactorily in terms of one, overall system, but should be considered as a set of subsystems, ..." (PT:280), I am not so sure of the dispute in (b). I know, for instance, that within American English there is a subsystem which maintains the distinction of /d/ and /t/ but there is another that has merged them as /d/ in words like rider/writer. Now, if I were to keep the two subsystems but reject (b) and follow its opposite, I would have to insist that there were also two subsystems in grammar corresponding exactly to the two subsystems in phonology, one-to-one. But no one, not even Lyons, has ever proved this to be the case for an actual analysis. Would Lyons be willing to face the consequences of such an analysis, if there were one?

I think it is more likely that (1) there is an overall grammatical system in any language with two or more phonological subsystems in it or (2) there are two or more grammatical sybsystems in the language with multiple phonological subsystems but the two sets of subsystems have one-to-many, rather than one-to-one, relationships. In either case, a certain amount of autonomy in the phonology of the language is inevitable.

Second, we have also learned that generative phonologists are equally wrong, if not more so than American structuralists, because they have ignored, without exceptions in their writings, the existence of subsystems. Their insistence on a uniform entity, even though they talk about dialects and what not, is under the pressure of their framework within which they must operate. For instance, while structuralists (or taxonomic phonemicists) can accommodate the two subsystems of rider/writer very easily without altering anything in their phonemic inventory--all they need to do is admit that there are such subsystems--generative phonologists are stuck with rules like Chomsky's (20), (23), (28), and (29) which are at best good for nothing, be they stated in terms of features or not; in other words, generative phonologists could not get out of this kind of straight jacket to account for new facts, even if they knew that subsystems exist, unless they started everything from the beginning with a clean slate. In this connection, it

should also be stressed that if Lyons supports generative phonology as he does transformational grammar (1970), he has no room for criticism regarding the orthodox American view.

Now the verdict! If systematic phonemics is not morphophonemics or if generative phonology is not taxonomic phonology, then what is it? In light of what has been discussed, I cannot help but say that it is a set of impressionistic rules, erratic and dangerously loose at times, that exploits taxonomic phonemes to the fullest extent in the name of morphophonemes, sometimes of archiponemes, in order to handle very limited data devoid of intonations and tones with results that conceal even important segmental facts. There are signs, however, that younger generative phonologists have come to realize this serious weakness and are, therefore, striving for improvement by incorporating such taxonomic ideas as the phoneme and the morpheme into their work. But what a detour it was!

Footnotes

1. This paper was presented at the inaugural meeting of The Linguistic Association of Canada and the United States, August 1974, at Lake Forest College, Lake Forest, Illinois.

2. For the rest of this paper, the following convention will be used: To an article that has been included in Makkai's anthology, I shall refer directly first with its original publication date and then with the page number(s) to PT, especially when a portion of that article has been quoted in the text. No cross-reference of this kind will be employed, however, when a cited article is not a part of PT.

3. Wang's article (1967) is something else; it contains factual errors but follows taxonomic tone phonemes. That is, it takes tone phonemes as a point of departure and then breaks them down to constituents. It will carry us too far afield, if we discuss it. Therefore, we shall not be concerned with it in this presentation.

4. The tonal designations are as follows (Peng 1967:198):

/ˊ/: High to next toneme or end of segment.
/-/: (Unmarked except after another toneme marked over a vowel.) Mid to next toneme or end of segment.

/ˆ/: High followed by low to end of segment, or by mid if /-/ follows later in the segment.

/ˋ/: Low to next toneme or end of segment.

5. In these examples, the transcription is in line with that of Mathews' Chinese-English Dictionary (1960). Numerical superscripts from 1-4 are used to designate the tones of the language.

6. See fn. 4 for the descriptions.

7. The third tone in a citation form is a long rise with a dip at the onset, but forms a short low fall when followed in a sequence by any tone other than another third tone.

8. See fn. 7 for non-sandhi alternations of tones in Mandarin.

9. See Johns (1969) for an example of fluctuation (PT: 549-50).

10. I did a little investigation, just for the fun of it, at the International Christian University among native speakers of American English who are either faculty members or students, concerning pair-words like rider/writer, ladder/latter, leader/

liter, lidder/litter, in citation and context. It is of interest to note that clear distinctions are made for the first three pairs with stops (mostly in citation) or flaps (mostly in context) but that every single speaker pronounced patio with a d-flap while only some would flap the d in radio. Also of interest is the fact that the speakers were aware of the flap in patio by saying "I think I pronounced the word with a d." Another intriguing fact is that they fluctuated among [d], [t], and plain [t] for litter but seldom did this for liter.

11. People living in North Bergen across the Hudson River opposite to New York City would pronounce burden [boydən] and Jordan [ǰoydən], just as they pronounce their town [noyθ] [boygən]. For those who have a post-vocalic r, these two words do not apply here.

12. Diachronically, then, it may be said that while the etymon of atom is /ætəm/, which has become /ædəm/ in American English, it has come down as is in British English or even Australian English. As a matter of fact, this is what should be posited in comparative method (cf. Lehmann 1973:76).

13. This is in line with their equally ridiculous features [±Germanic], [±Greek], [±Romance] (Chomsky and Halle 1968:373) and [+Sino-Japanese], [+native], [-foreign], [+onomatopoeia] (McCawley 1968). If they wish to play with etymology in generative phonology, I suggest that they go all the way, such as [±Hebrew], [±Arabic], [±Swahili], [±Chinese], [±Japanese], etc., for English and [+English], [+Russian], [+Spanish], [+French], [+Malay], [+Ainu], etc., for Japanese! Or do they think that these sources are not as important? How about [+Cantonese via Japanese] for tycoon but [+Chinese] for typhoon and [+Cantonese] for chop-suey? And how about [+Ainu] for Mt. Fuji and thousands of place names in Japan? Are they not morphemes also?

14. I have used parentheses instead of single quotes, for the English glosses and omitted a footnote concerning the transcription Hockett has used.

15. Note that both solutions are in line with the traditional American (taxonomic) phonemics, the second being on a par with Hockett's suggestion mentioned above.

16. Householder (1965) has an interesting comment here. He asked: "The question here is, what kind of length is represented? If it is instrumentally measured length, i.e., if it is the case that all instances of writer spoken so as to be accurately distinguished from instances of rider by a speaker of this dialect have an [a] segment which is measurably shorter, and nothing else different, why this is obviously nonsense" (PT:452).

17. But they are not in complementary distribution (cf. Cuba vs. tuber.

18. Bloch (1948) took the former case on the ground "that [ə˞] is congruent ... with [əl] but not with [lə], ..." (PT:;94).

19. In the case of pretty, perhaps a different treatment must be accorded, such as /pərti/ [pə˞ti], if we follow Bloch's suggestion, for certain speakers for whom metathesis has already taken place in the word. A similar instance of metathesis involving r is horse < hros which is well-known in historical linguistics. In this connection, would generative phonologists like Chomsky and Halle assign [+Anglo-Saxon] as a morpheme feature to take care of the metathesis that had happened to the word horse? If so, what about pretty? Are they going to assign [+Late Latin via Anglo-Saxon] to it? Cf. AS prættig and LL practicus. And what about personal names, such as Chairman Mao, Premier Chou, and Premier Tanaka which have become well-known in English-speaking parts of the world, and millions of lesser names?

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On the Scope of Negation in Hindi*

Tej K. Bhatia
University of Illinois, Urbana

0. INTRODUCTION. The topic of negation has interested logicians and philosophers both in the East and the West, in modern as well as ancient times. Negation has been a favorite topic for extensive and insightful discussions in various Indian schools of philosophy and logic such as the Naiyayika, the Vaiśeṣika, the Buddhist and the Jain (see Bhattacharya, 1965). In these schools of thought, interesting questions such as what negation is, the reality of negation, negation as a mental phenomenon, etc., have been discussed with great curiosity and depth. Also, in the West, much attention has been focused on this topic in the fields of philosophy and logic. However, surprisingly enough, it failed to attract the attention of linguists and grammarians[1] in any serious sense until very recently. The disinterest of linguists in this topic stems probably from the apparently simple syntax of negation. It was assumed that from a given positive sentence X, a negative sentence could be derived by placing a negative particle in an appropriate position and adjusting the order of elements in the sentence, if necessary.

In modern linguistic literature, negation became an attention-catching topic following the study of Klima (1964). Further research on this topic has disclosed that the very simple surface representation of a negative sentence stems from an enormously complex semantic representation.

Negation plays an important role in linguistic theory. It is used frequently as one of the confirmatory tests to examine suggested analyses of topics such as complementation, causativization, etc. It also serves as an evaluation procedure for competing analyses. Since the domain of negation is wide, the investigation of this phenomenon requires, as a prerequisite, the careful formulation of other syntactic processes such as complementation, causativization, reflexivization, participialization, etc.

The focus of this study is mainly the scope and semantics of negation (henceforth, NEG). In contemporary linguistics, although the nature of deep structure is becoming increasingly semantics-oriented and abstract, the treatment of the semantics of NEG has failed to account for its highly abstract behavior. The theory will greatly benefit from a competent investigation of this phenomenon, since a negative sentence demands a very abstract and semantically-oriented representation at the deep level. It has been shown in section 3 and 4 of this paper that any constituent (such as NP, adverb, post-positional phrase, or adjective) can be within the scope of NEG. While accounting for the scope of NEG it is argued that Klima's analysis, the higher verb analysis and other proposed analyses fail to account for the logical scope of NEG. Hence, a search for an analysis which could account for the logical scope of NEG at the level of deep structure continues. It is also shown that the concept of sentential and constituent NEG are based on the phonological/morphological reflexes of NEG rather than its syntactic or semantic properties. Questions such as what NEG is and

under what circumstances something can be negated, and topics such as double NEG, partial NEG and negative-prefixed items, NEG raising, the position of NEG in the deep structure are outside the domain of this study.[2] Logically, any lexical item has correlation with (absolute) NEG. For example, any lexical item, say L, can be expressed as [+x] and [-1,2,3...n] in the lexicon. The relation of negation with the process of lexicalization is beyond the scope of this paper.

Before I discuss the scope of NEG in Hindi it may not be irrelevant to briefly review the treatment of NEG in the linguistic literature, both in Hindi and English.

0.1. In the earlier transformational framework, NEG was treated as a transformation. Chomsky in Syntactic Structures derives a negative sentence with not from its corresponding underlying positive sentence. The first detailed and extensive study on NEG in English presented by Klima (1964) introduces NEG in the deep structure. Klima's historic article presented the most complete syntactic analysis of NEG ever attempted. He did not incorporate the semantics of NEG in his work. However, Carden (1967) and Lakoff (1969) attempted to describe the semantics of NEG. Constructions with 'Double Negation' were discussed in detail by Baker (1970). In the latest work on NEG from MIT, Lasnik (1972) noted the shortcomings of earlier works and attempted to develop a unified theory of NEG by developing the insights presented by the syntax-oriented work of Klima and the semantics-oriented works of Lakoff and others. In this work, Lasnik presents the interaction of the scope of NEG with some quantifiers and adverbs. However, he has not extended his analysis to other elements of the sentence such as prepositional phrases, modifiers, etc. He presents two theories -- DET-THEORY and PRE-S THEORY -- to account for the scope of NEG in a sentence and concludes that PRE-S THEORY is more powerful than DET-THEORY. However, he does admit that 'there is no natural way that the scope of Not can be determined in the deep structure within Pre-S analysis' (Lasnik, 1972:66). This shows the shortcomings of existing analyses.

0.2. The earlier research on Hindi on this topic can be grouped under two headings: In the first group we have description of NEG in traditional grammars. In the second group we have modern linguistic descriptions. In Greaves (1933)[3], Kellog (1938)[4], Scholberg (1940)[5], Guru (1952) and Sharma (1958) NEG has not been treated in any detail and no insightful observations have been made. Some examples of negatives, however, are given while discussing voice and verbs. In Sharma's grammar, there are only two places where NEG is mentioned. While discussing Imperatives (p. 63) he points out that NEG particles nə and mət are used in imperatives. In his chapter on Syntax, there is only one sentence about NEG: 'A negative sentence with terminate present may denote unwillingness, refusal, etc.' (Sharma, 1958:124).

Bahl's grammar (1968) is based on some selected topics of Hindi grammar. Unfortunately, NEG is not one of them. While discussing participle phrases, verbs and compound verbs, negative sentences are never brought into view. Thus, questions such as whether or not the operator is dropped in negative sentences are not discussed in these grammars.

In short, in the traditional grammars of Hindi, only the following two points about NEG have been discussed: First the distribution of

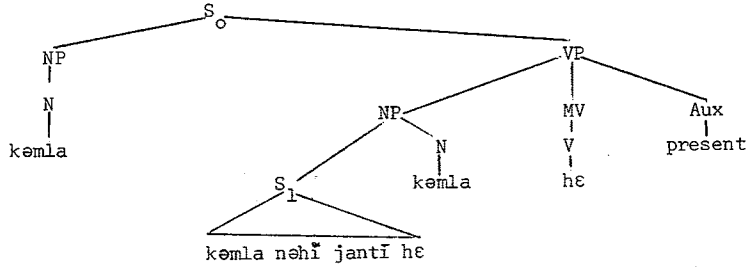
NEG-particle under adverbs or imperatives; second, of hɛ-deletion (as an Aux and/or Copula).

In a recent description of Hindi, Kachru (1966) presents a linguistic description of NEG. In Kachru (1966) she has discussed the process of deriving negative sentences and has shown the relationship between the NEG-particle and other constituents of S in the transformational-generative framework. She points out that the choice of negative element results in the deletion of Aux and operator at the surface level. In Kachru (1966:106-7) she noted that some derived participial phrases cannot be negated.

J. Singh (1968) begins with the description of negative prefixes at the morphological level. At the syntactic level, preverbal and post verbal occurance of NEG-particle and NEG in coordinate sentences are discussed with various examples. The section on 'Negation of Specific Elements' includes the process of negating derived participle phrases. This description is mainly presented in what is generally termed the structural framework.

Sahay's paper (1969) (in Hindi) does not incorporate the insights presented by J. Singh. Not only is the analysis presented superficial, but the data is wrong and the examples incomplete. Sahay begins his paper with a discussion of the distribution of NEG-particles which has already been presented in traditional grammars (for example, in Sharma's grammar). The most confusing part of this study is the use of wrong data and incomplete examples which do not account for the intuitions of native speakers. He claims that participle phrases can be negated and gives the examples nə kərte hue 'NEG doing', nə khate hue 'NEG eating', but one cannot say: *mɛ nə khate hue pəṛh rəha tha. Similarly he says V + kər - type phrases can appear with NEG-particle nə; however, he does not admit that they are not the negatives of the corresponding positive phrases.

I. Singh's (1970) is the latest available paper on this topic. Although this paper adopts the transformational-generative framework, his analysis is far from satisfactory. He presents a very sketchy description of sentential NEG, Negative Nominalization and Double NEG. In the section 'Negative Nominalization' he tries to derive NEG-prefixed items such as ənjan 'ignorant' from an S x nəhĩ janta. For example, the sentence kəmla ənjan hɛ 'Kalma is ignorant' is derived from the following structure (p. 39).



This analysis has the following problems: First, no motivation has been presented for the above deep structure. Second, the process

of the lexicalization (ənjan 'ignorant') has been left unaccounted for. Third, how two occurances of kəmla are deleted is left unexplained. Fourth, how pronominalization and reflexivization are to be blocked is not discussed. In short, his analysis is not plausible. Furthermore, no motivation has been presented for the derivation of ənjan from an embedded S_1.

1.0. NEG-PARTICLES AND THEIR EFFECTS ON THE SURFACE FORM OF AN S. Before starting the discussion on the scope of NEG in a simple sentence, I would like to discuss Hindi NEG-particles and their distribution at the level of surface structure since these negative particles have been used in the examples discussed in this paper.

At the level of surface structure, three negative particles can appear in preverbal position in a negative sentence. nəhĩ is the representative NEG-particle. The other two NEG-particles appear in specific environments. mət appears with the imperative form of a verb, whereas nə shows up in imperative optative, participial, gerundive and conjoined constructions.

The choice of the negative element has the following effects on the surface forms of sentences.

i. The NEG-particle occurs between the predicate complement and V-hona 'to be', e.g.,

1(a) vəh əccha admĩ hɛ.
he good man is
He is a good man.

(b) vəh əccha admĩ nəhĩ hɛ.
He is not a good man.

ii. An optional rule, which deletes the Aux 'hɛ' following the aspect markers -t, -a and rəh- operates in the negative sentences.

2(a) vəh jata hɛ.
he go Aux
He goes.

(b) vəh nəhĩ jata
he NEG go
He does not go.

iii. If a compound verb has two elements, main verb and operator (the main verb is present in its uninflected stem form and the operator is inflected for number, gender and tense), then if the NEG-particle occurs, the stem of the operator is dropped and the number, gender and tense markers are attached to the stem of the main verb.

3(a) mẽ ne kitab pəṛh lĩ
I AG verb 'operator'
case stem
'study'
I have read the book.

(b) mẽ ne kitab nəhĩ pəṛhĩ
NEG stem + (number, gender and tense)

*(c) mẽ ne kitab nəhĩ pəṛh lĩ.

In 3(a) pəṛh is the stem of the infinitive form of the verb pəṛhna 'to study/read, and lĩ 'took' is the operator in which /ĩ/ is past tense form. It is the number, gender and tense suffix /ĩ/ which is

attached to the stem of the main verb when the NEG-particle appears in the sentence 3(b).

2.0. THE TERM 'SCOPE OF NEGATION'. Klima used the notion 'in construction with' to describe the scope of NEG. He attempted to account for the phenomenon of the scope of NEG by setting forth two types of NEG, namely sentential NEG and constituent NEG. In the case of sentential NEG, the sentence NEG is 'in construction with' merely the constituent members of the NP which appear in a lower-level branching of the S.

In the NEG as a higher verb analysis the scope of NEG is expressed in terms of 'command' relationship. Since in this analysis, only the sentences can be negated, the whole S which is dominated by or commanded by a NEG falls under the scope of NEG.

Lasnik (1972) employs 'scope' to mean NEG of a particular item. In other words, when an item is being negated, it is within the scope of NEG. I will use the term 'Scope of negation' as used by Lasnik, since Klima's distinction between sentential and constituent NEG is phonologically and morphologically motivated and fails to account for the logical scope of NEG. Consider the following examples:

4(a) yəh səty nəhĩ hɛ.
it true not is
It is not true.

(b) yəh əsətya hɛ.
it neg. prefix true is
It is false.

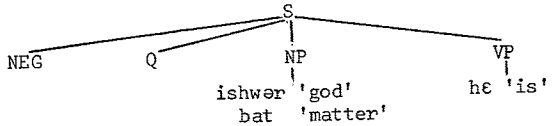
According to Klima's analysis 4(a) is an example of sentential NEG while its paraphrase 4(b) is an example of constituent NEG.

3.0. SCOPE OF NEGATION IN THE SIMPLEX SENTENCE. As I pointed out earlier, NEG appears in preverbal position in simple sentences whether a sentence is an example of sentential NEG or phrasal NEG. This fixed position of NEG at the surface level gives rise to ambiguities in Hindi. However, any Hindi sentence can be disambiguated by the context. Consider the scope of NEG in the following simple sentences.

5) kya ĩshwər nəhĩ hɛ?
Q god not is
Is there no god?

6) kya bat nəhĩ huĩ?
Q matter/ talk not took place
Did it not happen?

The above sentences are examples of sentential NEG. In a tree structure diagram they can be represented in the following way:



3.1. Although the NEG-particle always appears in preverbal position, the scope of NEG does not remain static. It changes immediately if any other verb is substituted in the above examples. For example:

7) vəh nəhĩ aya.
he NEG come
He did not come.

8) usne kitab nəhĩ pəṛhī
he AG book NEG read
case
He did not read the book.

In the above examples 7 and 8, the scope of NEG is limited to the VP of the S. In examples such as 8 NEG can either be limited to V or to the NP (object) of the VP. 8 can be expanded as in 8(a).

8(a) usne kitab nəhĩ pəṛhī əxbar pəṛha
He did not read a book, (he) read a newspaper.

The scope is restricted to NP (object) of the VP in 8(a).

In sentences such as the following it may extend to the VP as a whole.

8(b) usne kitab nəhĩ pəṛhī, sirf pətr likha
he AG book NEG read only letter wrote
case
He did not read the book, only wrote a letter.

3.2. In adverbial constructions, NEG may cover either the whole VP, or only the adverbial phrase under its scope. Usually, in the negated sentences the latter has precedence over the former. Consider the following examples:

9) mẽ roz nəhĩ pəṛhta.
həmesha
əksər
I daily NEG study
always
often
I don't study daily/always/often.

A comparison of the above sentences with 8 shows that the presence of the adverbial phrase plays a significant role in defining the scope of NEG. In sentence 9, the verb is out of the scope of NEG whereas the adverbial phrase falls under its scope. Speakers of Hindi can paraphrase the above sentence as follows:

10) mẽ pəṛhta to hũ lekin roz/həmesha/əksər nəhĩ.
I study but not daily/always/often.

If the verb had been covered by the scope of NEG in the paraphrase of 9 (i.e. sentence 10), it would have been impossible to delete the identical verb in the underlying second conjunct of the conjoined sentence 10.

An identical process can be noticed in the following example with a locative adverb.

11) vəh ghər mẽ nəhĩ pəṛhta.
he house in NEG study
He does not study at home.

Like sentence 9, sentence 11 will be understood as 'he studies but not at home'. nəhĩ cannot negate the verb in this sentence. Sentence 11 is parallel to the Hindi sentence given below:

12) vəh pəṛhta hɛ məgər ghar mẽ nəhĩ
he studies AUX but house in NEG
He studies but not at home.

When the NEG-particle appears with durative adverbs, the scope of NEG is either extended from adverbial phrase to full verb phrase or remains, as in the case of other adverbs, limited to the adverbial phrase. Consider the following example in this regard:

13) usne do ghənṭe tək kam nəhĩ kiya
he AG two hours until work NEG did
case
He did not work for two hours.

Sentence 13 can be interpreted in two ways: (a) where the scope of NEG is limited to the time adverb only; (b) where the scope of NEG covers the entire VP. Reading (a) is obvious in sentences such as (14).

14) usne kam to kiya pərəntu do ghanṭe tək nəhĩ
he AG work Emp. did but two hours until not
case particle
He worked but not for two hours.

Reading (b), i.e., negation of the entire VP is present in the following sentences.

15(a) jəb tum cəle gəye usne do ghənṭe tək kam nəhĩ kiya
after you left he AG two hours until work NEG did
case
After you left, he did not work for two hours.

15(b) usne do ghənṭe tək kam nəhĩ kiya, uske bad kərne ləga
he AG two hours until work NEG did that after doing started
case
He did not work for two hours; after that he started doing it.

Geis (1970) suggests that negatives and adverbs are to be treated as higher verbs according to the 'abstract analysis' suggested by George Lakoff.

> "In an abstract analysis in which the negation element underlying <u>not</u> is claimed to be a subject-embedding predicate in deep structure, all constituents referred to as being 'in the scope of' the negative are within the embedded sentence subject of negative' (Geis, 1970:5).

According to this analysis, durative adverbs in the example given below are treated in the following way.

16) ram ne do ghənṭe tək tumharī təsvīr nəhĩ dekhī
ram AG two hours until yours picture NEG saw
case
Ram did not look at your picture for two hours.

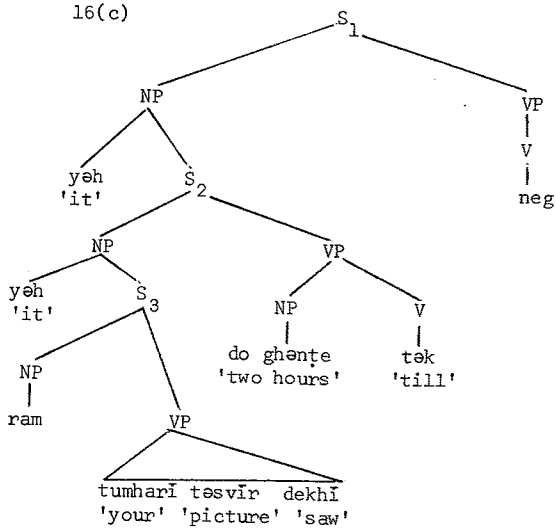
According to Geis the above sentence 16 is ambiguous. It can be either interpreted as 16(a) or as 16(b).

16(a) yəh bat nəhĩ ki ram ne tumharī təsvīr do ghənṭe tək dekhī
It matter not that ram AG your picture two hours until saw
case
It is not a fact that Ram looked at your picture for two hours.

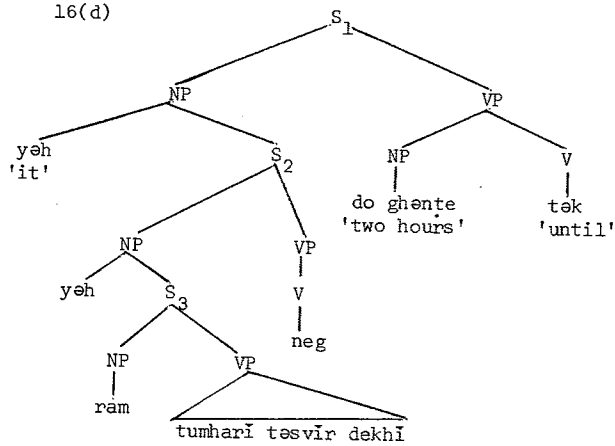
16(b) do ghənṭe tək ram ne tumharī təsvīr nəhĩ dekhī
For two hours Ram did not look at your picture.

To explain this ambiguity, Geis draws tree-diagrams corresponding to 16(c) and 16(d) given below:

16(c)



16(d)



According to Geis, only sentences can be negated. Sentence 16 consists of two simplex sentences of which one is 16(e).

16(e) ram ne tumharī təsvīr dekhī
Ram looked at your picture.

This can be negated as a unit. Since we cannot negate any non-sentential unit such as 'do ghənṭe tək', the adverbial phrase is placed out of the scope of NEG at the level of deep structure.

Another reading is obtained by negating the whole sentence, including the adverbial phrase, i.e. 16(f).

16(f) ram ne do ghənṭe tək tumhari təsvir dekhi.

In 16(f), the adverbial phrase constitutes a part of the larger sentence.

According to this analysis, the negative sentences with instrumental and manner adverbials are also ambiguous in two ways, like those with many other adverbials. Thus sentences such as 17,

17) mẽ ne bleḍ se seb nəhĩ kaṭa
I AG razor with apple NEG cut
case blade
I did not cut the apple with a blade.

are ambiguous, having the meaning either of the NEG of the whole sentence (i.e., S with the adverbial phrase) or the sentence without the adverbial phrase in 17(a) and 17(b) respectively.

17(a) mẽ ne bleḍ se seb nəhĩ kaṭa
I did not cut the apple with a razor blade.

17(b) mẽ ne seb nəhĩ kaṭa
I did not cut the apple.

However, the adverbial phrase in itself cannot be negated. Contrary to the claim of this analysis, Hindi speakers intuitively interpret this sentence as negating the adverbial only. To any native speaker of Hindi, sentence 17 means: 'The apple was cut but not with a blade.' The above sentence 17 can be paraphrased as 17(c):

17(c) mẽ ne seb kaṭa məgər bleḍ se nəhĩ
I cut the apple but not with a razor blade.

or by 17(d) mẽ ne seb bleḍ se nəhĩ cakũ se kaṭa
I AG apple razor with NEG knife with cut
I cut the apple not with a razor blade, (but) with a knife.

Consider another example with manner adverb:

18) mẽ ne savdhanī se dərvaza nəhĩ khola
I AG carefully door NEG opened
I did not open the door carefully.

The above sentence according to Geis has two readings:

18(a) mẽ ne dərvaza nəhĩ khola
I did not open the door.

or

18(b) mẽ ne savdhanī se dərvaza nəhĩ khola
I did not open the door carefully.

In 18, the adverb alone cannot be negated. However, a native speaker of Hindi will not hesitate to substitute the following sentence for 18.

19) mẽ ne shayəd əsavdhanī se dərvaza khola
I AG probably NEG prefix door opened
carefully
I probably opened the door carelessly.

The fact that 18 can be understood as 19 is sufficient evidence that the adverb can itself be negated. Geis' analysis provides no explanation for the fact that in 18 it is not asserted that the door was not opened; 18 asserts simply that the door was opened, but that the action was not performed in a careful manner.

The paraphrase equivalence of 18 and 19 brings out clearly the inadequacy of the proposed classification of sentential NEG and constituent NEG. Following Klima, in many works sentences such as 18 are usually cited as examples of sentential NEG and sentences with an item

+ negative-prefix as constituent NEG (see Klima, 1965:295, Kim, 1967: 122; McGloin, 1972:1). Any grammar which treats 18 and its paraphrase 19 as distinct fails to capture the generalization and suffers from the inadequacies of PS grammars (which fail to establish a legitimate relationship between an S and its paraphrase). In short, in the linguistic literature, the distinction between sentential NEG and constituent NEG is phonologically or morphologically motivated and lacks either syntactic or semantic support. Furthermore, it fails to capture significant generalizations in the language.

The above discussion does not apply to adverbs only. An identical process can be observed when participial forms are substituted for adverbs, as participial forms, like adverbs, fall under the scope of NEG. Consider the following sentences.

20) tum pita jĩ ko ate hĩ tõg mət kəro
you father obj. arriving brother NEG do
marker
Don't bother your father the moment he arrives.

(Notice that the phonetic shape of the NEG partical <u>mət</u> indicates that it originates in the higher S.)

21) vəh pəṛh kər nəhĩ soya
he studied having NEG slept
He slept without studying.

22) vəh pəṛhte pəṛhte nəhĩ soya
he while reading NEG slept
It was not while reading that he fell asleep.

In sentences 20, 21, and 22 the main verb is out of the scope of negation. For example, sentence 20 does not mean 'don't bother your father.' Rather it seems to imply 'you can bother your father, but don't do it the moment he arrives.' Similarly in 21 and 22, although the NEG particle appears before the verb <u>soya</u> 'slept', the participial forms <u>pəṛh kər</u> 'having studied' and <u>pəṛhte-pəṛhte</u> 'while reading' fall within the domain of negation.

The above discussion leads us to the following conclusions: One, in Geis' framework, the participials should be treated as higher verbs as opposed to the embedding analysis of participials. However, if such an analysis is accepted, it will fail to account for the fact that the participial alone can fall within the scope of negation.

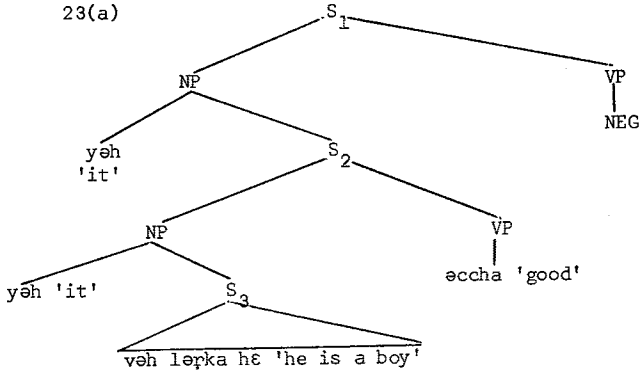
Two, if the embedding analysis of participial forms is adequate, then NEG will be treated as the higher verb of the embedded S. Such treatment will need an extra rule of NEG-raising in order to arrive at the desired surface representation. However, this treatment is not free from the shortcomings discussed above. Since NEG-raising is not applicable to factive verbs (Bhatia, 1972:43). In such instances NEG-raising results in a semantic change. However, if this rule is blocked, ill-formed sentences will be generated. Thus in any case, this proposal suffers from inadequacies. Hence Geis' analysis is descriptively inadequate and fails to account for the scope of NEG. Furthermore, its claims are counter-intuitive.

3.3. Also, Geis' treatment fails to account for the congruency of behavior between adverbs and verbs on the one hand, and adjectives and nouns on the other, with respect to the scope of NEG. In Geis' analysis, adjectives and nouns in the VP are treated like adverbs. That is why sentences 23 and 24 can be represented by 23(a) and 24(b) respectively.

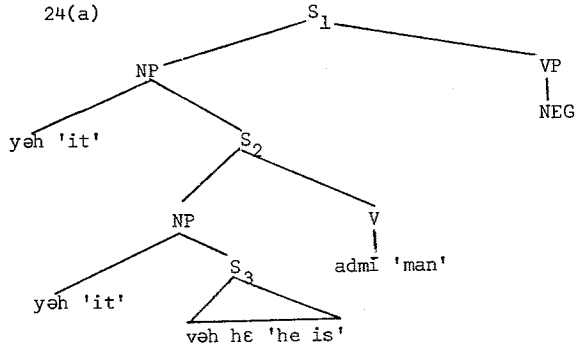
23) vəh lərka əccha nəhĩ hɛ
that boy good NEG is
That boy is not good.

24) vəh admī nəhĩ hɛ
he man NEG is
He is not a man

23(a)



24(a)



Sentences 23 and 24 do not negate the existence of <u>lərka</u> 'boy' and <u>admī</u> 'man' respectively. The native speaker of Hindi will paraphrase 23 and 24 by 23(b) and 24(b) respectively.

23(b) vəh lərka to hɛ lekin əccha nəhĩ
he boy EMP is but good NEG
He is a boy but he is not good.

24(b) vəh admī nəhĩ kuch ɔr hɛ
he man NEG something else is
He is not a man, (he is) something else.

However, Geis' deep structure representation makes the wrong claim that both S_2 and S_3 come within the scope of NEG whereas we have seen that only the adjective <u>əccha</u> 'good' and the noun <u>admī</u> 'man' are negated in 23 and 24 respectively. In other words Geis' deep structure

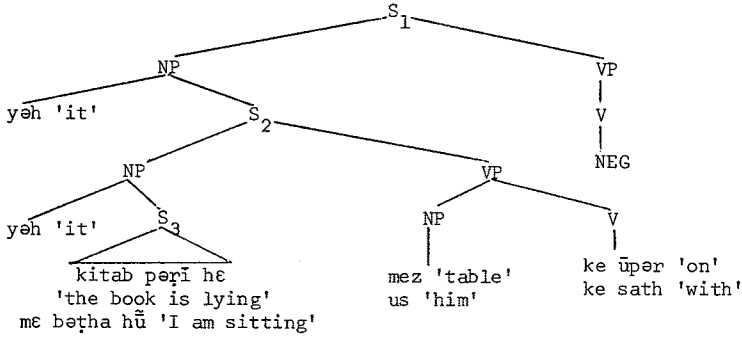
representation fails to account for the NEG of an adjective or noun and on the other hand, makes the wrong claim that S_3 is within the scope of NEG.

3.4. If a postpositional phrase occurs in an S, the NEG-particle has the potential to negate either the entire postpositional phrase or merely the postposition. Examine the following:

25) kitab mez ke upər nəhĩ pəṛī thĩ
the book table on NEG lying was
The book was not lying on the table.

26) mẽ uske sath nəhĩ beṭha
I him with NEG sat
I did not sit with him.

According to the abstract anlaysis, prepositions are a special sort of verb. If this analysis is extended to postpositions, it would treat them as predicates of higher sentences.[6] For example, 25 and 26 will be represented as

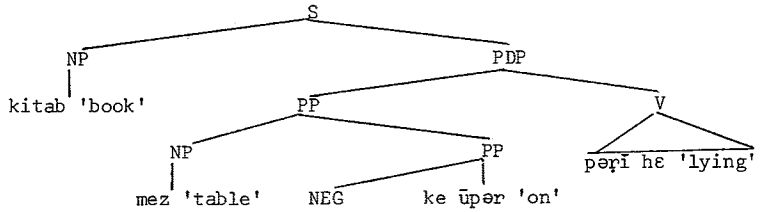


Notice that the above treatment faces two difficulties. First, the S_3 such as kitab pəṛī thĩ
mẽ bɛṭha hũ
should not come under the scope of NEG although NEG commands S_3 according to the above analysis.

Second, the above analysis does not permit the NEG of postpositional phrases only (or their postpositions) although in one interpretation of the above examples the postpositional phrase mez ke ūpər 'on the table' and us ke sath 'with him' are negated. Not only can the postpositional phrases be negated but in example 27 only the postposition can be negated. Consider the following sentence:

27) kitab mez ke upər nəhĩ nĩce pəṛī hɛ
the book table on NEG under lying is
The book is not lying on the table, but under the table.

A representation such as the following can account for such examples. It can clearly indicate the scope of NEG as understood by native speakers of Hindi. The representation of sentence 27 is given below.



This analysis can accurately account for the intuitions of the native speaker, since the native speaker will understand sentence 25 as 27. Thus Geis' treatment of prepositions does not account for the scope of NEG in postpositional phrases satisfactorily if it is extended to it.

3.5. Verbs and nouns display a parallel behavior with respect to the scope of NEG when they are modified by adverbs and adjectives respectively. When the NP of any VP is modified by a determiner, numeral, demonstrative pronouns or adjective, the scope of NEG covers the modifier only. For example, in Hindi:

28) mɛ̃ sundər ləṛkõ̃ se nəhĩ̃ mila
do
un
əcche
I handsome boys with NEG met
two
those
good

I did not meet the handsome/two/those/the good boys.

The above sentences imply that 'I met some boys, but I did not meet the handsome/two/these/good boys.'

The scope of negation extends to the subject too. For example

29) yəhã koĩ nəhĩ̃ aya
here someone NEG came
Nobody came here.

3.6. Hindi has two emphatic particles, to and hĩ. I shall discuss their roles in specifying the scope of NEG below.

3.6.1. The particle to: The emphatic particle to characteristically attracts NEG to the phrase which it follows. Observe the following examples in Hindi:

30(a) mɛ̃ ne to use khət nəhĩ̃ likha
I AG EMP him letter NEG wrote
case part
I did not write him a letter.

(b) mɛ̃ ne use to khət nəhĩ̃ likha.
I did not write him a letter.

(c) mɛ̃ ne use khət to nəhĩ̃ likha
I did not write him a letter

(d) mɛ̃ ne use khət likha to nəhĩ̃
I did not write him a letter.

Notice that sentences 30(a-d) are identical except that to follows different phrases. In 30(a) to follows subject NP mɛ̃ 'I'. Even though the NEG-particle appears in the pre-verbal position, the verb and its object NP's are clearly outside the scope of NEG. to has an

absolute potential to attract NEG to the subject NP, i.e., mẽ 'I'. The sentence will be interpreted in the following manner.

30(e) kisĩ ne use khət likha mẽ ne nəhĩ
someone AG him letter wrote I AG NEG
case case
Someone wrote him a letter, but I did not.

Thus, the speaker knows that the action of 'writing a letter to him' was performed but it was not 'I' who performed this action. Similarly in 30(b) to restricts the scope of NEG to only the indirect object 'him'. The speaker says that he admits that he has written a letter but not to him, rather to somebody else. 30(c) indicates that khət 'letter' is under the scope of NEG and in 30(d), the scope of NEG is limited to the verb only since to follows the verb phrase.

3.6.2. The particle hĩ: When the emphatic particle hĩ occurs in the negative clause the effect of NEG is neutralized. Examine the following sentences in this regard:

31) mẽ vəhã nəhĩ gəya
I there NEG went
I did not go there.

32) mẽ hĩ vəhã nəhĩ gəya
I only there NEG went

(a) It was I who did not go there.

(b) It was not only I who went there.

31 is a negative sentence while 32 is ambiguous. The ambiguity is clear from its translations into (a) and (b). In 31 the action of 'going there' was not performed. However, in 32(b) the speaker does not intend to say so. The speaker wants to convey that the action of 'going there' was not only performed by him but by others too. Thus, 32(b) shares a common logical underlying structure with the following sentences:

33) mẽ hĩ vəhã nəhĩ gəya dũsre log vəhã gəye
I EMP there NEG went other people there went
I did not go there, other people went there.

34) mẽ hĩ nəhĩ dũsre bhĩ vəhã gəye
Not only I but also others went there.

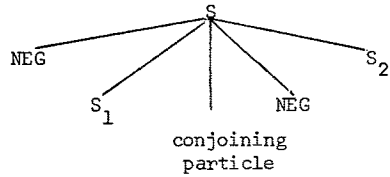
Hence, in the reading of 32(b), 32 does not function as a negative sentence. hĩ attracts NEG to it (as is clear from the surface structure of 34 and neutralizes it. However, in the other reading 32(a) where hĩ functions as sirf 'only/emphatic', hĩ fails to attract NEG to itself and, as a result, the scope of NEG covers VP under its scope. Thus this reading of 32(a) is negative and can be represented by the following Hindi sentence.

35) sirf mẽ hĩ vəhã nəhĩ gəya
only I EMP there NEG went
Only I did not go there.

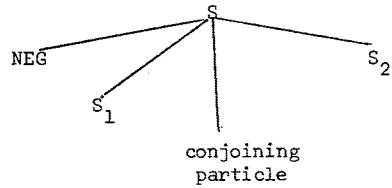
In other words, the sentences with emphatic particles are ambiguous with a negative as well as a positive reading when the NEG-particle occurs in the surface representation. In the positive reading, NEG is attracted towards hĩ and is neutralized and in the other reading hĩ fails to attract NEG and, hence the NEG covers the VP under its scope.

4.0. SCOPE OF NEG AND CONJOINED SENTENCES. NEG in conjoined sentences can appear in structures like the following:

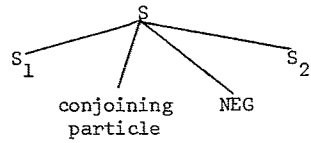
A.



B.



C.



Two NEG-particles which can appear at the surface structure level in the above structures are nəhĩ and nə. nə can appear only in structure A but nəhĩ can appear in all of the above structures. nəhĩ sometimes appears in the post verbal position but usually appears before the verb. nə appears usually in the pre-sentential position. Thus, these two NEG-particles can appear in different positions. This contradicts the assumption that NEG appears only in the preverbal position (as at the surface level NEG keeps moving from one place to another). Thus, the NEG placement before a verb in deep structure can neither account for the scope of NEG accurately nor will it be syntactically justified. Consider the following examples of conjoined sentences.

36) nə mɛ̃ bazar gəya ɔr nə mɛ̃ ne bazar dekha
I did not go the the market nor did I see the market.

37) nə mɛ̃ bazar gəya ɔr nə mɛ̃ ghər gəya.
I did not go to the market nor did I go to the house.

38) nə mɛ̃ bazar gəya ɔr nə suresh bazar gəya.
I did not go to the market nor did Suresh go to the market.

36(a) mɛ̃ bazar nəhĩ gəya ɔr mɛ̃ ne bazar nəhĩ dekha
I did not go to the market and I did not see the market either.

37(a) mɛ̃ bazar nəhĩ gəya ɔr mɛ̃ ghər bhī nəhĩ gəya.
I did not go to the market and I did not go to the house either.

38(a) mɛ̃ bazar nəhĩ gəya ɔr suresh bhī bazar nəhĩ gəya
I did not go to the market and Suresh did not go to the market either.

In sentences 35, 36 and 37, the NEG-particle appears in the pre-sentential position. Nevertheless, the scope of NEG extends to the verb, adverb and the subject NP respectively. If the NEG-particle nəhĩ of the simplex sentence is selected at the surface level we get sentences 36(a), 37(a), and 38(a) respectively.

When nə is selected, the conjunct is reduced and the NEG-particle precedes the constituent or constituents it negates.

37(b) mɛ̃ nə bazar gəya ɔr nə ghər.
Neither did I go to the market nor to the house.

38(b) nə mɛ̃ bazar gəya ɔr nə Suresh
Neither did I go to the market nor did Suresh.

4.1. Aspect and Negation: In coordinate structures like 40 and 41 when the NEG particle appears in the post verbal position the scope of NEG is limited either to the verb or to the aspect only. Examine the following:

39) mɛ̃ skūl mẽ pəṛhta nəhĩ khelta hũ
I school in study NEG play AUX
I don't study in school but I play.

40) mɛ̃ skul mẽ pəṛhta nəhĩ məgər əb pəṛhũga
I school in study NEG but now will study
I didn't study in school but now I will.

*41) mɛ̃ skūl mẽ nəhĩ pəṛhta məgər khelta hũ[8]
I don't study in school but play.

42) mɛ̃ skūl mẽ nəhĩ pəṛhta məgər ghər mẽ pəṛhta hũ
I don't study in school but at home I do.

*43) mɛ̃ skūl mẽ pəṛhta nəhĩ məgər ghər mẽ pəṛhta hũ.
I don't study in school but I study at home.

*44) mɛ̃ skūl mẽ nəhĩ pəṛhta məgər pəṛhũga.[9]

*45) mɛ̃ skūl mẽ pəṛhta nəhĩ khela
I do not study in school but played.

46) mɛ̃ ne kitab pəṛhī nəhĩ paṛh rəha hū
I did not read the book, I ram reading.

In sentences 39, 40, and 46 the NEG-particle appears in the post-verbal position. The scope of NEG in sentence 39 is limited to the verb pəṛh 'study' only, while in sentences 40 and 46 it is limited to present and past respectively.

When the NEG-particle occurs in the post-verbal position in coordinate structures like 40 or 41 it is not possible to negate both the verb and aspect tense. That is why sentence 45 is ungrammatical.

Consider example 42 where NEG is placed in the preverbal position and its scope covers the adverb skūl mẽ 'in school', but when NEG is moved to the post-perbal position only the verb or the aspect and tense is under its scope. Adverbs in such cases fall outside its scope. That is why sentence 43 is ungrammatical. In the case of 44 the scope of NEG covers the adverb and, thus, aspect cannot be negated. Therefore, it is an ungrammatical sentence.

Thus, when the NEG-particle follows the verb the scope of NEG is either limited to the verb or to the aspect and tense of that verb. NEG cannot cover the verb and aspect and tense both under its scope in corrdinate structures such as 40 and 41.

5.0. CONCLUSION. Although the theory of negation has undergone radical changes in recent linguistic analysis, the treatment of NEG is still far from satisfactory. I have shown that the concepts of

sentential and constituent NEG are superficial and are phonologically or morphologically motivated, and thus lack syntactic or semantic motivation. I have presented some examples to demonstrate that the presence of a NEG-particle in preverbal position does not necessarily imply sentential NEG, for the following reasons:

One, the logical scope of NEG may not cover the entire sentence. Such a negativized sentence could be paraphrased using a NEG prefixed item.

Two, the proposed analysis fails to account for the ambiguity of certain negativized sentences in Hindi.

Three, in respect to certain negativized sentences, NEG is neutralized when pronounced with normal rhythm. (See examples 32-5 above.)

Four, in conjoined sentences the NEG-particle can either appear in preverbal position or in front of the phrase/item which it logically negates.

Five, certain negativized sentences could be paraphrased or translated by corresponding NEG incorporated items in English.

Six, the analyses discussed above fail to capture certain very productive generalizations in the language, e.g., the marked parallelism in negativized sentences in the behavior of modifier/adjectives and adverbs modifying nouns and verbs, respectively.

Furthermore, since the concept of sentential NEG permits only one NEG per S, it is inadequate to account for the phenomenon of 'Double NEG'. I have also demonstrated that the treatment of NEG so far not only fails to account for the logical scope of NEG but also makes misleading and counter-intuitive claims. Although, in contemporary linguistic theory, the nature of deep structure is becoming more and more abstract and semantically oriented, and questions such as representing presuppositions at the level of deep structure are being discussed, the treatment of NEG is still a long way from achieving descriptive adequacy.

In this paper, I have attempted to demonstrate the non-static behavior of NEG in Hindi simplex and conjoined sentences. I have also shown that the appearance of NEG in the preverbal position in simplex sentences gives rise to ambiguity. In the deep structure, the so-called 'sentential NEG' or any other analysis proposed so far, fails to account for this ambiguity since any constituent (VP, adverbs, postpositional phrase, postposition, NP, etc.) can be under the scope of NEG. Hence, the concepts of Sentential and Constituent NEG have to be discarded in favor of an analysis which can account for the logical scope of NEG in a sentence.

NOTES

*I am grateful to Professor Y. Kachru and S.N. Sridhar who read an earlier version of this paper. My thanks are also due to R. Pandharipande and A.H. Siddiqui who have helped me sharpen my native speaker's intuitions about Hindi.

[1]Jesperson (1966) is an exceptional case. He has discussed this topic in great detail.

[2]For a discussion on these topics see Bhatia (1972).

[3]Greaves discussed negation under 'miscellaneous adverbs'. He has presented the distribution of NEG-particles, nəhĩ, nə and mət; kəbhī nəhĩ 'never' and kyõ nəhĩ 'why not' have been included in this discussion, but he does not mention NEG in his table of contents (383-4).

[4]Kellogg mentioned NEG at different places, for example: a) NEG-particle nə in disjunctive conjunctions, p. 39; b) distribution NEG-particles nə and mət under 'The Imperatives', p. 459; c) he makes a passing remark about Aux deletion in 'Syntax of the Indefinite Imperfect', p. 464. Similarly, another remark about copula deletion in 'Of the Copula', p. 500. d) Phrases like nəhĩ to 'otherwise' are discussed in a short paragraph, p. 534.

[5]Scholberg (1940) was the first to mention NEG in the table of contents. In the section of adverbs he lists a subsection 'Afirmation and Negation' but under this title he discusses only the distribution of NEG-particles. His other remark about hɛ deletion occurs on p. 228.

[6]In her paper M. Steffensen (1971:157-8) presented the following arguments for the treatment of postpositions as verbs on the basis of the Abstract Analysis suggested by G. Lakoff and adopted by J. Geis.

Both transitive verbs and postpositions have objects and exhibit co-occurrence restrictions with objects. These can be most parsimoniously stated on the postpositions and unless these surface level postpositions are posited as verbs, some explanation must be given for the fact that the same nouns are relevant in both cases.

Both verbs and postpositions undergo verb phrase and object deletion.

In the case of pronominalization of the object she does not feel that her arguments are so strong. With many adverbial objects of verbs, it could be argued that the postposition had been deleted by a late rule.

[7]One can also say: nə mẽ bazar gəya, nə ghər.

[8]This sentence is acceptable if skũl mẽ 'in school' is not implied in the second conjunct.

[9]However, the following sentence is well-formed:

mẽ skūl mẽ əbtək nəhĩ pərhta tha əb pərhũga
I school in until now NEG used to study now will study
Until now I did not study in school, but now I will.

since the adverb əbtək 'until' is within the scope of NEG.

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On describing the cluster prosody

T.D. Griffen
University of Florida

In word-initial consonant clusters of Modern Welsh, there are some interesting phenomena involving voicing and aspiration. According to the data of Fynes-Clinton (1913), when a liquid follows a word-initial voiceless plosive, it lacks voice. On the other hand, when a liquid follows a word-initial voiced plosive, it is itself voiced.

Now in many languages, including English and German, the notion of the occurrence of voice in a liquid following a word-initial plosive being dependent upon the occurrence of voice in the plosive may be supported by data; but in the absence of alternations between word-initial voiced and voiceless plosives, such a notion is not easy to demonstrate beyond a doubt (particularly for those who may not fully trust the principles of nonconstrastive distribution). In Welsh, however, there is such a system of alternations. This 'mutation system', as it is commonly termed by those working within the field, is a system of initial consonant gradation, and it is quite extensively covered in Morgan 1952 and adequately covered in Morris Jones 1913 and in most Welsh textbooks.

One type of mutation is termed 'soft mutation' or 'lenition', and it affects several different types of segments (see Griffen In press a). In soft mutation (among several different types of alternation), a voiceless plosive is realized as the voiced cognate. As we see from the data below, when a voiceless 'radical' is realized as its voiced soft mutation form, the previously voiceless liquid following the radical is also realized as the voiced form. Thus, we find the following:

radical	soft mutation	gloss
pl̥aid	blaid	party 432
pl̥um	blum	lead 435
pr̥en	bren	tree 442
pr̥i:od	bri:od	married 443
tl̥aud	dlottad (equative)	poor 534
tr̥ai	drai	ebb-tide 539
tr̥evn	drevn	order 543
kl̥edi	gledi	hardship 262
kl̥ox	glox	bell 267
kr̥a:s	gra:s	dry 292
kr̥i:st	gri:st	Christ 296

(The circle below the /r/ and the period below the /l/ both represent voicelessness in the notation of Fynes-Clinton 1913. I have extracted the page number after the gloss of each item for the convenience of those who wish to check the data. More examples can be found in this source and through field work.)

An initial plosive can also be followed by a nasal, always /n/. When the nasal follows the voiceless plosive, however, it is voiced, as we see in the following data, also from Fynes-Clinton 1913:

radical	gloss
pnaun	afternoon 436
pnelin	elbow 436
knaud	flesh 273
kniu	knee 275

There are two more mutations in the system which affect voiceless plosives. One is the 'spirant mutation' in which a voiceless plosive is realized as a voiceless fricative (or spirant). When a liquid follows the initial voiceless fricative in spirant mutation, it is voiced, as in the following:

radical	spirant mutation	gloss
pl̥y:	fly:	feathers 435
pr̥i:s	fri:s	price 444
tr̥aus	θraus	trans 540
kl̥ebar	xlebar	chatter 262
kr̥ødi	xrødi	krowdy (fiddle) 299

(Fynes-Clinton's notation /y/ represents a high central vowel.)

Thus, we can see that the presence of voice in the liquid is determined by the presence of voice in the plosive, but it is not dependent upon the presence or absence of voice in the initial fricative.

The final type of mutation is called the 'nasal mutation'. In this mutation, the radical plosive is realized as the nasal cognate. Now if the plosive is voiceless, so is the nasal cognate, and the aspiration of the plosive is heightened in the voiceless nasal to the degree of 'glottal fricative' (see Jones 1969). This is perhaps the most interesting of all the phenomena under study, for when the consonant undergoing nasal mutation is followed by a liquid or by a nasal, the liquid or nasal is not only voiceless, but it is followed by the glottal fricative (the aspiration of the initial segment). Thus, we find the following alternations:

radical	nasal mutation	gloss
pl̥e:θ	m̥lhe:θ	plaiting 432
pnelin	m̥nhelin	elbow 436
tr̥əmdar	n̥rhəmdar	dead of night 553
kl̥y:st	ŋ̥lhy:st	ear 272
kr̥ombil	ŋ̥rhombil	gizzard 298
knottyn	ŋ̥nhottyn	diminutive epithet 275

(The circle below the nasal represents voicelessness.)

I should first like to address the data within the generative phonological mode of description. I shall not discuss the changes inherent in the mutated consonants themselves, as I have treated these elsewhere (see Griffen In press a and In press b) and shall treat them in great detail in Griffen Forthcoming. I wish rather

to concentrate on the specification of the feature voice in the liquids and nasals in initial clusters.

First of all, the generative phonological approach to such a problem would assume that either /r/, /l/, and /n/ or /r̥/, /l̥/, and /n̥/ are the underlying segments. In accordance with the universal interpretive conventions of Chomsky and Halle 1968, the underlying segments would clearly be /r/, /l/, and /n/, as these are sonorants. Considering the effects of a voiceless plosive on a nasal and the effects of a voiceless fricative on a liquid, this would appear to be a reasonable assumption.

The data derived from the effects of soft mutation can be handled quite neatly by a rewrite rule as follows:

$$C \longrightarrow [\alpha\text{voiced}] \;/\; \begin{bmatrix} C \\ \alpha\text{voiced} \\ -\text{continuant} \end{bmatrix} \begin{bmatrix} \\ +\text{sonorant} \\ +\text{consonantal} \end{bmatrix}$$

We can call this rule a 'voice assimilation rule'. Such a rule is quite a common phenomenon.

Unfortunately, this rule will not work. In addition to the forms given above, we also find such words as /stra:yn/ 'strain' (504), /stremp/ 'smudge' (505), and /stry:d/ 'street' (506), in which the sonorant not only follows one voiceless consonant, but it follows two voiceless consonants, the second (immediate) of which is a plosive, and yet it is voiced.

What is involved here is aspiration. The /s/-plus-plosive environment is a well-known deaspirating environment in English (see Gleason 1961:263) as well as in Welsh (see Fynes-Clinton 1913:xxii). From the data it appears that the aspiration of the initial consonant is what causes the following sonorant to be voiceless, and in /s/-plus-plosive clusters this aspiration is absent (compare Kurath 1964:74). Thus, it is one feature of the plosive which governs the following sonorant. In order to treat this phenomenon with this insight, we would have to systematize the notion of a cluster prosody within the generative framework. Although Fromkin (1965) attempted to do just that, her lead was not followed within the school, and consequently there has been no method for incorporating this notion into generative rules.

The situation, however, can be remedied by specifying the above rule so that the preceding consonant cannot be preceded by an /s/. This avoids the prosodic approach, at least for the moment. Such a rule can be posited as follows:

$$C \longrightarrow [\alpha\text{voiced}] \;/\; \begin{Bmatrix} V \\ \# \end{Bmatrix} \begin{bmatrix} C \\ \alpha\text{voiced} \\ -\text{continuant} \end{bmatrix} \begin{bmatrix} \\ +\text{sonorant} \\ +\text{consonantal} \end{bmatrix}$$

This will still bring us problems, notably where a syllable boundary is relevant to the aspiration even when an /s/ or other consonant precedes the boundary and where stress patterns may interfere with aspiration. Nonetheless, I do not believe that the situation can get so far out of hand with the phenomena treated to this point that we could not patch this up into a

workable rule, regardless of extreme complexity.

With the nasal mutation, however, we really come upon a problem. In order to generate the /pnelin/ → /m̥nhelin/ rule, for example, without resorting to reference to prosodies we would have to posit the following rules:

(1)	Nasal mutation	pn → m̥hn
(2)	Aspirate metathesis	m̥hn → m̥nh
(3)	Voice assimilation	m̥nh → m̥n̥h

The first rule is morphophonological and quite general. The metathesis rule could be handled transformationally within the framework of Chomsky and Halle 1968. The voice assimilation rule would have to follow metathesis, as /h/ is not, strictly speaking, a consonant. The aspirate metathesis rule is central to a process segmental explanation, but it is laden with problems. How does a feature of one segment become a segment in its own right capable of metathesis? Moreover, how 'natural' is a rule which takes such aspiration from one segment and simply adds it to the next?

Now the voice assimilation rule would need further revision, for it only affects liquids. In order for it to apply here, however, it would also have to apply to nasals. But as we find in /pnelin/, it does not and cannot affect nasals. I do not wish to contest the notion that with tortuous use of angled brackets and other such notational devices a set of rules could be forged which would account for the data. What I should want to contest, however, is any notion that would insist that the data be made, by force if necessary, to adhere to a framework, even to the point of 'aspirate metathesis'. As I see it, the question is not 'How can we make it work?', but rather it is 'How can we accurately and insightfully account for the data?'

As I state above, the voicing of the sonorant can be linked with the aspiration of the preceding plosive. Given the notion that, by definition, that which is aspirated is voiceless (at least, in Welsh), we can account for the data by adopting the notation of prosodic analysis (see Firth 1948), as follows:

$$\frac{h}{PL} \qquad \frac{h}{PN} \qquad \frac{h}{NS}$$

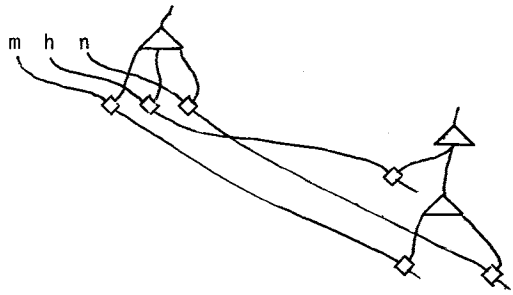
This is to say that aspiration from an aspirated (voiceless) plosive dominates the following liquid but not a following nasal; while that of an aspirated nasal dominates the following sonorant and the onset of the vowel. In order to account for the deaspirating effects of the initial /s/ within a process-oriented framework, we can describe this as a prosodic process which restricts the domination of aspiration in the particular environment.

The events stated so succinctly in prosodic analysis cannot be incorporated into the current item-and-process framework because of the singulary nature of the generative rewrite rule. The entire generative outlook on deep-to-surface processes is restricted

both by framework and by notation to single items. Here, however, we find single features dominating more than unitary segments.

In order to handle this by singulary rule, the generative phonologist would have to consider a larger-than-segment phonological approach--perhaps changing entire syllables at a time. As demonstrated by Liberman 1970, the structure of the syllable will have to be considered eventually (as analysis by synthesis approaches the surface), but if an item-and-process framework is to expand the scope of its operation (that is, the item) from features in bundles of features to wholesale changes in bundles of bundles of features, the economy of this process framework, which has been boasted so fervently in the past decade, will be lost.

As we turn our attention to the stratificational mode of description, we find an entirely different situation relative to the Welsh data. Unlike generative theory, which maintains no distinct levels but generates phonetic output through rules applied to systematic phonemic input, stratificational theory can account for the data by a shift in dominance between the phonemic level and the hypophonemic, phonetic level. Thus, while aspiration is a linearly-ordered phonemic entity in the one stratum, it is a hierarchically dominant entity on the next stratum, as in the following diagram:



In a recent manuscript, Lockwood has incorporated the Firthian notion of prosodic analysis into the stratificational model by means of the 'quasi-step matrix'. Now the step matrix, itself a departure from notational purity, would render the nasal mutation form of /pnelin/ as follows:

m̥	n̥	h	e	l	i	n
Ns	Ns	Gl	Vo	Lt	Vo	Ns
As	As	As	Vc	Vc	Vc	Vc
La	Ap		Mi		Hi	Ap
			Fr		Fr	

The step matrix fails to account for the fact that speech is continuous--not the uttering of one bundle of components after another. Lockwood's quasi-step matrix can take this fact and the prosodic nature of aspiration into account as follows:

m̥	n̥	h	e	l	i	n
Ns		Gl	Vo	Lt	Vo	Ns
As			Vc			
La	Ap		Mi		Hi	Ap
			Fr		Fr	

Not only does this innovation incorporate the Firthian notion of prosodic domination, but it also incorporates the Prague notion of contextual neutralization and assimilation of oppositions (see Trubetzkoy 1969). The result is a more accurate description of the relationships involved. Of course, in order to arrive at a description of this type, it was necessary to modify the network approach of stratificational notation. The network would have to be left somewhere, however, before the actual encoding of sound.

Returning once again to the framework of generative phonology, we can now approach the problem of accounting insightfully for the data by recognizing two things. Firstly, the most straightforward manner of accounting for the phenomena described in Welsh is through prosodic analysis. Secondly, stratificational linguists have found that it is possible to incorporate prosodic notions into the matrix. As generative phonology also has a matrix, the remedy should be obvious.

Within the generative phonological component, we can introduce prosodic processes which spread the specification of a particular feature in the matrix over other features in the matrix. For example, taking [+aspirated] to be [-voiced] by definition, we can reduce the specifications of affected segments. Thus, we can obtain the following:

	m̥	n̥	h	e	l	i	n
.							
.							
.							
aspirated	+	→		-	→		
.							
.							
.							

The rules governing such phenomena can be set down in the phonology in a manner similar to the prosodic notation cited above.

Furthermore, by omitting the occurrence of /h/ as a separate segment, we can manipulate the feature [aspirated] so as to capture the notion that this is a prosodic feature rather than a separate segment. In a process framework, this would be desirable as it would eliminate the present need for 'creating' a segment somewhere

in the derivation. In addition to prosodies, such a device can handle assimilation without the need for tortuously specified rules, such a those above (compare also Griffen 1974).

The only insightful manner in which generative phonology can account for the cluster prosody involves a modification of the strict framework of the item-and-process rewrite rules carried out on singulary segments. Incorporating such changes, then, will not be an easy task, for the changes represent evidence against theoretical purity and a justification of an eclectic approach to linguistics.

In spite of the fact that changing the theoretically pure foundations of generative phonology will probably not prove an easy task, such changes are inevitable, for, as Chomsky and Halle (1968) point out, even the phonetic detail must ultimately be generated. Now Liberman 1970 demonstrates that the actual phonetic event of speech is a complex code which requires its own grammar--the 'grammar of speech'; and as we can see by Liberman, Cooper, Shankweiler, and Studdert-Kennedy 1967, the often simultaneous nature of the grammar of speech contains elements which preclude the current generative methods. Thus, a shift in approach will have to be made if the claims to complete adequacy of the generative model are to be maintained.

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BOTH EITHER AND OR

Jean LeGrand
University of Chicago

In English, EITHER is used in three distinct ways. First, it may appear clause finally, as the negative polarity counterpart of TOO:

(1) a. I don't like Bourbon or Scotch, either.
b. I like Bourbon and Scotch, too.

Second, it may appear predisjunctively, as in (2). In this use it appears to parallel preconjunctive BOTH:

(2) a. Either the Cubs or the White Sox will win.
b. Both the Cubs and the White Sox will win.

Finally, EITHER is used as a pronoun or determiner, as illustrated in (3). In this use, EITHER appears to resemble the pronoun/determiner BOTH:

(3) a. You can't trust either of them.
b. You can't trust either boy.
c. You can trust both of them.
d. You can trust both boys.

In this paper, I will be concerned with this final EITHER, the one used as a pronoun or determiner, and its relationship to BOTH, AND and OR.

In 1970 Guy Carden wrote a paper called "The Deep Structure of BOTH", in which he addressed himself to the nature of the pronoun/determiner BOTH. Was it a quantifier, like ANY, ALL, and SOME, or was it derived from the conjunction AND?

Let me make clear at the outset how a conjunction differs from a quantifier. A conjunction relates two separate sentences. A quantifier says something about a variable, which may be present in a single sentence, or in a series of sentences. The point is, quantifiers and conjunctions may themselves be the same thing--McCawley (1972) argues that this is the case--but the sorts of environments quantifiers and conjunctions appear in are necessarily different.

In his paper, Carden concludes that the pronoun/determiner BOTH is a conjunction, rather than a quantifier. He claims that BOTH is syntactically derived from a conjunction in the manner sketched out in (4):

(4) a. x and y came. Optionally →
b. Both x and y came. Optionally →
c. Both (of them) came.

Carden concedes that deciding between a quantifier source and a conjunction source for BOTH is difficult: he observes that there is some fairly superficial syntactic evidence for each alternative. One crucial datum, however, caused him to decide that the pronoun/determiner BOTH is actually derived via a deletion transformation from preconjunctive BOTH. That datum is this:

The pronoun/determiner BOTH may not occur with symmetric predicates like MEET and KISS, while quantifiers like ALL, MANY, SOME, ANY, etc. may occur with symmetric predicates. That is, (5a.)--if it means anything at all--may not mean that the two people referred to by BOTH met each other. But in (5b.), the people referred to by ALL may be, and generally would be, understood to have met one another:

(5) a. ?Both of them met/kissed in the plaza at noon.
b. All of them met/kissed in the plaza at noon.

Now AND by itself frequently does conjoin the subject of a symmetric predicate, as illustrated in (6a.). But if preconjunctive BOTH precedes the conjoined subject, the reading is no longer symmetric: in (6a.), the natural reading is that John and Mary kissed each other; in (6b.), if (6b.) is grammatical at all, John and Mary did not kiss each other.

(6) a. John and Mary met/kissed in the plaza at noon.
b. ?Both John and Mary met/kissed in the plaza at noon.

It was this parallelism between preconjunctive BOTH and pronoun/determiner BOTH that made Carden decide that the latter was derived from the former.

This brings me to EITHER. The pronoun/determiner EITHER is parallel to the pronoun/determiner BOTH in three respects. First, the pronoun/determiner EITHER is pronounced the same way as the predisjunctive particle EITHER is, just as preconjunctive BOTH is pronounced identically to pronoun/determiner BOTH. Second, pronoun/determiner EITHER, like pronoun/determiner BOTH, may not occur with symmetric predicates:

(7) a. ?I don't think either of them met in the plaza. ≠
b. I don't think the one met the other in the plaza.

Third, pronoun/determiner EITHER resembles BOTH in that it may only refer to exactly two individuals or items, as shown in (8):

(8) a. Either of those two men can come.
b. *Either of those three men can come.
c. Both of those two men can come.
d. *Both of those three men can come.

On the basis of these similarities between EITHER and BOTH, it seems reasonable to conclude that if the pronoun/determiner BOTH is derived from the conjunction AND, then the pronoun/determiner EITHER must be derived from the disjunction OR.

But: I will now present five pieces of evidence--some of them only suggestive, others fairly conclusive--to show that pronoun/determiner EITHER cannot be derived from predisjunctive EITHER, nor from the disjunction OR itself.

First, as I just showed, the pronoun/determiner EITHER may only refer to exactly two items or individuals; but predisjunctive EITHER may, for most speakers, appear even when more than two sentences are conjoined, as in (9a.). Incidentally, this reflects a difference between predisjunctive EITHER and preconjunctive BOTH: for a majority of speakers (9b.) is ungrammatical:[1]

(9) a. That was said by either Lenin or Trotsky or Marx.
b. *That was said by both Lenin and Trotsky and Marx.

Second, predisjunctive EITHER may appear whenever OR appears: that is, predisjunctive EITHER appears in all the sentence types that OR appears in.[2] But the distribution of pronoun/determiner EITHER is quite limited: it appears principally in conditionals, negatives, and possibility and permission sentences. It may not appear in simple positive sentences:

(10) a. I saw either Lenin or Trotsky.
b. Either Lenin or Trotsky said that.
c. I never met either Lenin or Trotsky.
d. If you met either Lenin or Trotsky, tell me.
e. You can write a book about either Lenin or Trotsky.

(11) a. *I saw either/any of them.
b. *Either/any of them said that.
c. I never met either/any of them.
d. If you meet either/any of them, tell me.
e. You can write a book about either/any of them.

Notice that, as shown in (11), the distribution of EITHER is very similar to the distribution of ANY. I'll return to the ANY/EITHER relationship below.

So far it has been shown that the pronoun/determiner EITHER has a more restricted distribution that predisjunctive EITHER in two respects: it cannot occur in simple positive sentences, and it can never be used to refer to more than two items or individuals. These first two differences are disturbing, but perhaps the theory deriving pronoun/determiner EITHER from predisjunctive EITHER might be patched up so as to account for them. The next two differences I will discuss are, however, more serious: there are at least two sentence types in which pronoun/determiner EITHER is grammatically and semantically permissible, but in which the disjunction OR itself is anomalous.

The first and most conclusive of these sentence types consists of conditional sentences in which there is co-reference between the EITHER of the protasis and something in the apodosis, as illustrated in (12) and (13):

(12) a. Mary can swim the Channel, if either of those two can.
b. Mary can swim the Channel, if any of them can.
c. *Mary can swim the Channel, if John or Mary can.

(13) a. McCawley is right, if either of them is.
b. McCawley is right, if any of them is.
c. *McCawley is right, if (either) McCawley or Jackendoff is.[3]

(12a.), with EITHER, is fine, just as (12b.), with ANY, is. But (12c.), inwhich EITHER has been replaced by the disjunction "John or Mary", is incoherent.

If EITHER is treated as a quantifier, closely related to ANY, it becomes possible to explain this difference between EITHER and the disjunction which might be claimed to be the source of the pronoun EITHER. If EITHER is treated as a manifestation of the universal quantifier binding a variable in the protasis of the conditional, we come up with a semantically reasonable representation of the meaning of (12a.):

(14) a. Ax (IF x can swim the Channel THEN Mary can swim the Channel) =
b. (IF Ex (x can swim the Channel) THEN Mary can swim the Channel)
Range of x: Mary, John.

may be input to the Disjunction-to-EITHER transformation, for as shown in (11) above, not all disjunctions may be replaced by the pronoun EITHER.

If there were a nice simple rule for determining which disjunctions may become the pronoun/determiner EITHER, we might, other things being equal (which I've tried to show they aren't) be willing to accept the idea that the pronoun/determiner EITHER is derived from a disjunction.

Such a rule was in fact proposed by Lawrence Horn (1972). He made the claim that a disjunction might become the pronoun/determiner EITHER just in case that disjunction has an expanded paraphrase in the form of a conjunction. Such AND-paraphrasable ORs are called "affective ORs": the examples in (18), (19) (20) and (21) may make clear what is meant by this term:

(18) a. I didn't meet Lenin or Trotsky. =
b. I didn't meet Lenin and I didn't meet Trotsky.

(19) a. If you like Bourbon or Scotch, raise your hand. =
b. If you like Bourbon, raise your hand, and if you like Scotch, raise your hand.

(20) a. You can drink Bourbon or Scotch. =
b. You can drink Bourbon and you can drink Scotch.

(21) a. I drank Bourbon or Scotch. ≠
b. I drank Bourbon and I drank Scotch.

The ORs in (18) - (20) are affective ORs: these sentences have AND-paraphrases. But (20a.) does not contain an affective OR, for (20b.) is not a paraphrase of (20a.). Corresponding to Horn's claim, as shown in (22), the disjunctions of (18)-(20) may be replaced by EITHER; but the disjunction of (21) may not:

(22) a. I didn't meet either (one).
b. If you like either (one), raise your hand.
c. You can drink either (one).
d. *I drank either (one).

Unfortunately, Horn's simple rule for determining which disjunctions may be transformed into pronoun/determiner EITHER is inadequate. Horn claimed that all and only affective ORs might be replaced by the pronoun/determiner EITHER. This is not the case, however, as (23) shows:

As is shown by the logical equivalence of (14a.) and (14b.), (14a.) correctly states that if someone can swim the Channel, Mary can.

The reason for the strangeness of (12c.) may also be made clear by expanding (12c.) according to the logical formula for expanding ORs in conditionals; that formula is shown in (15):

(15) (IF A OR B) THEN C = (IF A THEN C) AND (IF B THEN C)

Applying formula (15) to (12c.) produces (16):

(16) If John can swim the Channel then Mary can swim the Channel and if Mary can swim the Channel then Mary can swim the Channel.

(16) is redundant and tautological; since (16) means what (12c.) means, (12c.) must also be unacceptably redundant and tautological. (In fact, I find (12c.) so strange that without application of a formula such as (15) I would be unable to discern what, if anything, (12c.) might mean.)

I can think of no way in which a theory which derived pronoun/determiner EITHER from a disjunction might be patched up so as to account for this difference between the disjunction and the pronoun/determiner. On the other hand, as has just been seen, this difference is accounted for automatically if EITHER is a quantifier.

Factive emotive verbs, like REGRET, provide a second example of the same sort of thing. For some speakers, pronoun/determiner EITHER and OR differ in that the former may appear in the complement of factive emotive verbs, while the latter may not. In my speech, for example, (17a.) is a fine sentence, just as (17b.), with the quantifier ANY, is. But I find (17c.) extremely strange:

(17) a. I regret that either of them came.
b. I regret that any of them came.
c. ??I regret that John or Mary came.

(17c.) might perhaps be spoken if "John or Mary came" were an echo of a previous utterance, but I doubt that it would otherwise be acceptable. Yet (17a.) is entirely acceptable even when it is not an echo-comment.

The final piece of evidence that militates against deriving the pronoun/determiner EITHER from a disjunction is rather complicated, but its implications are far-reaching. (For what these implications are, see LeGrand (1974).)

If the pronoun/determiner EITHER is to be derived from a disjunction, it is necessary to specify just which disjunctions

(23) a. I was the first/last person in my family to study either language.
b. I was the first/last person in my family to study any language.
c. I was the first/last person in my family to study Latin or Greek. ≠
d. I was the first/last person in my family to study Latin and I was the first person in my family to study Greek.

EITHER, like ANY, is perfectly acceptable in "first to" and "last to" type sentences, as is shown by (23a.). OR is also fine in such sentences, as shown by (23c.). But the OR in (23c.) is not affective: (23d.) is not a paraphrase of (23c.).

Horn's simple rule for accounting for the distribution of EITHER is insufficient. We must conclude that the relationship between pronoun/determiner EITHER and disjunctions is a complicated one; this is turn strongly suggests that it is incorrect to treat that relationship by means of transformation deriving the one from the other.

I have shown that there are five problems, some of them serious, which must somehow be handled in order to derive the pronoun/determiner EITHER from a disjunction. The alternative is to treat EITHER as a quantifier. It is tempting to claim that pronoun/determiner EITHER and ANY are manifestations of the same underlying element, EITHER being used to refer to exactly two, ANY being used in all other cases. However, there are two differences between EITHER and ANY.

The first is that ANY may appear with symmetric predicates while, as mentioned above, EITHER may not:

(24) I don't know whether any/*either of them are similar.

Second, ANY may be triggered by a following relative clause; EITHER may not:

(25) a. Any of the three who says he met Lenin is lying.
b. *Either of the two who says he met Lenin is lying.

I cannot at present account for these differences between ANY and EITHER. Perhaps future research will provide an explanation for the fact that ANY and EITHER, which are identical in most respects, differ in this way.

Footnotes

1. I am aware of one additional respect in which BOTH and EITHER differ: as illustrated in (A), EITHER may occur with "the other" while BOTH may not:

(A) a. I don't believe that either man likes the other.
b. I believe that each man/*both men like(s) the other.

2. Some speakers tell me that for them, predisjunctive EITHER marks the OR of a disjunction as being unambiguously exclusive. This is not true in my speeck; for me, the only way to indicate that OR is unambiguously exclusive (rather than ambiguous between inclusive and exclusive readings) is to stress the OR.

For those speakers from whom predisjunctive EITHER is associated with exclusive OR, an additional difference between predisjunctive EITHER and pronoun/determiner EITHER exists: for pronoun/determiner EITHER is not unambiguously exclusive. This is illustrated by (B):

(B) If either of them comes, let me know.

(B) does not unambiguously say that only if one of them comes are you to let me know, and that if both of them come, other instructions are to apply.

3. It was brought to my attention following the reading of this paper that certain conditionals with co-reference between a disjunction in the protasis and some element of the apodosis are markedly improved when predisjunctive EITHER is present. For example:

(C) a. If either of them is right, it's McCawley.
b. *If McCawley or Jackendoff is right, it's McCawley.
c. ?If either McCawley or Jackendoff is right, it's McCawley.

What is it that EITHER does in (Cc.) that causes (Cc.) to be an improvement over (Cb.)? It was suggested to me that this improvement might be due to the fact that for many speakers, predisjunctive EITHER marks a disjunction as being exclusive. I reject this explanation for two reasons. First, it is entirely unclear to me why the presence of an exclusive OR in (Cc.) should have the effect of making (Cc.) relatively acceptable. Second, for all speakers, stressing OR makes that OR exclusive. But if the OR of (Cb.) is stressed, no significant improvement over (Cb.) is obtained:

(D) *If McCawley <u>or</u> Jackendoff is right, it's McCawley.

I suggest as an alternative that the effect of EITHER in

(Cc.)--and in many other sentences as well--is to indicate that no other options exist. Thus (Cc.) says something like:

(E) If I'm stuck with the fact that either McCawley or Jackendoff is right, I'll pick McCawley.

Note that the data given here refute the claim made by Ross (1967) that predisjunctive EITHER, like preconjunctive BOTH, is simply a redundant manifestation of the underlying conjunction. While it may not be clear what predisjunctive EITHER does, it is clear that predisjunctive EITHER does something.

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TESTING RULES IN FUZZY GRAMMAR*

Bernard Mohan

University of British Columbia

I. Fuzzy Grammar

The aim of this paper is to put forward a model for rule functions in 'fuzzy grammar' and present the results of testing the model on the well-formedness judgments of informants. I will argue that specifying rule functions is important for the theoretical and empirical development of the concept of Fuzzy Grammar, and claim that a particular psychometric theory offers both an appropriate model for rule functions and a solution to measurement problems arising in testing.

Lakoff (1973) proposes a model for grammar, called 'fuzzy grammar', in which the well-formedness of sentences is not the standard dichotomy of well-formed/ill-formed but is rather a matter of degree, conceptualized as a scale ranging from 1 (completely well-formed) to 0 (completely ill-formed). The degree of well-formedness of a sentence depends on the rules which applied in its derivation. Accordingly, a grammatical rule in a fuzzy grammar will have associated with it a rule function defining the degree of well-formedness of its output. The degree of well-formedness depends on the rule involved and on factors entering into the rule. Lakoff claims that 'hierarchies' or ordering relations exist within certain factors. If it is possible to establish that hierarchies manifest themselves in a regular fashion across different rules, then significant generalizations can be captured by the fuzzy grammar model which may not be statable in the standard dichotomous model.

Lakoff illustrates his conception with an analysis of a rule of adverb-preposing into higher clauses. There is a hierarchy of adverbials, including (A1) time adverbs and (A2) place adverbs. This hierarchy is described more exactly as the degree to which the element affected by the rule (i.e. the preposed adverb) is a member of the adverb category. There is also a hierarchy of predicates whose complements have different degrees of 'islandhood', including (B1) 'likely' and (B2) 'realise'. Variation along the adverb category hierarchy and the island hierarchy is shown in (1a-d).

(1) a. A1B1 Tomorrow it's likely that Sam will eat his breakfast.
b. A2B1 ?In Berkeley it's likely that Sam will eat his breakfast.
c. A1B2 ?Tomorrow I realise that Sam will eat his breakfast.
d. A2B2 *In Berkeley I realise that Sam will eat his breakfast.

A third hierarchy involves predicates which 'trigger' adverb-preposing in their complements. Among these are (C1) 'likely' and (C2) 'realise'. Lakoff observes that the hierarchies for trigger and island match up exactly, where they can be compared. 1a' and 1c' show variation along the trigger hierarchy.

(1)a'. A1C1 It's likely that tomorrow Sam will eat his breakfast.
c'. A1C2 I realise that tomorrow Sam will eat his breakfast.

(1a-d) are considered to contain the best trigger (a deleted performative verb of saying) and 1a' and 1c' are considered to contain the best island (no embedded S). There are thus three relevant factors, the adverb category, island and trigger hierarchies, in the well-formedness of the sentences given and they are combined by a function which is associated with the rule of adverb-preposing, represented in (2a), which says that well-formedness (wf) for adverb-preposing sentences is a function of the values for each of the three hierarchies.

A similar analysis is offered for a rule of topicalisation where the trigger and island hierarchies are assumed to behave as in adverb-preposing. Here, though, the category hierarchy is the degree to which the element affected by the rule (i.e. the topicalised element) is a member of the noun category, and the rule function is that associated with topicalisation. This is represented in (2b).

(2)a. Adverb-preposing: wf $= f_1$(A category, island, trigger)
b. Topicalisation : wf $= f_2$(N category, island, trigger)

f_1 and f_2 in (2) represent the two different rule functions.

Consider (3a-d) where the island hierarchy varies across the rules of adverb-preposing (R1) and topicalisation (R2).

(3)a. R1B1 Tomorrow it's likely that Sam will eat his breakfast.
b. R2B1 ?Kittens it's likely that Sam hates.
c. R1B2 ?Tomorrow I realise that Sam will eat his breakfast.
c. R2B2 *Kittens I realise that Sam hates.

(3a-d) can be represented by Matrix 1 whose cells contain hypothetical degrees of well-formedness, whose rows contain different rules and whose columns contain different degrees of islandhood.

		ISLANDHOOD	
		B1	B2
Rule	R1	.7	.5
	R2	.6	.4

Matrix 1

As (2) implies, the well-formedness values in the first row of the Matrix are the result of f_1(A category, island, trigger) with the category value and the trigger value held constant, and similarly for the second row. Matrix 2 shows the analysis of each cell of

Matrix 1 in terms of these functions and constants.

	B_1	B_2
R_1	f_1(A category, B_1, trigger)	f_1(A category, B_2, trigger)
R_2	f_2(N category, B_1, trigger)	f_2(N category, B_2, trigger)

Matrix 2

While Lakoff specifies a rank ordering for certain items on each of the three hierarchies, he does not specify the form taken by either of the rule functions f_1 and f_2, nor does he indicate any constraints on the form such rule functions can take. The nature of rule functions constitutes a central problem for this model of grammar. From the viewpoint of theory construction, it is obvious that if there are no constraints on the general form of rule functions, a device of unknown power and complexity has been introduced into a theory of grammar. From the viewpoint of empirical testing, it is clear that if the form taken by a particular rule function is not specified, predicted values on hierarchies cannot yield predictions of well-formedness values for sentences, since the intermediary operation of the function is unknown. There are therefore strong theoretical and empirical reasons for attempting to specify the form of rule functions.

The question of empirical testing raises further difficulties. Given a specification for a particular rule function, it is also necessary to solve certain measurement problems if this specification is to be tested on the data of informants' judgments. Suppose that an informant gives a judgment of the well-formedness of a sentence of 3(a-d). It may be assumed that the informant takes the values given on each of the hierarchies, category i, island j, trigger k, and the rule function, f_1 and derives a well-formedness value, wf. On the basis of wf, he gives a judgment of well-formedness, J, as in (4):

(4) $\left.\begin{array}{l}\text{Category i}\\ \text{Island j}\\ \text{Trigger k}\end{array}\right] \rightarrow f_1(\text{category i, island j, trigger k}) \rightarrow \text{wf} \rightarrow \text{J}.$

Ideally, three measurement problems must be solved:

(i) to measure wf on an equal interval scale on the basis of J;
(ii) to measure the values on each hierarchy on an equal interval scale;
(iii) to test the hypothesised specification of the rule function, f_1.

In this study, attention will be limited to problems (i) and (iii). Problem (i) is simply the problem of the validity of informant judgements as a measure of grammatical intuitions. It is present whether

the claims of fuzzy grammar or the claims of dichotomous grammar are being tested, but it is likely to be more acute when an informant is asked to judge the degree of well-formedness of a sentence rather than to make a dichotomous judgment. Problem (iii) is unique to fuzzy grammar and I know of no proposed solutions in the linguistic literature to date. Without solutions to these measurement problems, it is not clear how specific claims about fuzzy grammar rules could be tested with validity and precision.

II. Rule Function Model

Some recent work in psychology indicates both a model which can be applied to rule functions and a solution to the attendant problems of measurement. Anderson (1968, 1970, 1971) offers a model for information integration. It is supposed that the subject makes a judgment based on a set of stimuli or pieces of information. Broadly speaking, the judgment based on the set is assumed to be a linear function of the values of the items:

(5) $J = C + \Sigma w_k s_k$,

where J is the judgment, C a constant, and s_k and w_k are the value and weight of the kth item in the set. It is assumed that each stimulus item can be represented by two parameters, s and w. Of these, s represents the value of the item along the dimension of judgment, and w represents the weight or importance of that item in the overall judgment. The total effect of two or more informational stimuli is the weighted sum of their scale values. (The constant, C, allows for an arbitrary zero in the response scale. For simplicity, it will be ignored.) (cf. Anderson 1968 pp. 732-3)

The model has been successfully applied to a range of information tasks including personality impressions, decision making, and psychophysical judgment. Judgement of the well-formedness of sentences could also be regarded as an information integration task, since the informant must integrate scale values on the different hierarchies entering into sentences in order to arrive at a well-formedness value. Applying the model in detail, an s value will correspond to a scale value of a given point on a hierarchy, and the role of a rule function will be to assign weights to the different hierarchies entering into the rule, thus assigning a degree of relative importance to each hierarchy. Thus each s, a hierarchy value, is multiplied by the weight, w, associated with that hierarchy and the results are summed. (2) can now be revised as indicated in (6).

(6) a. Adverb-preposing:
$wf = (w_1 \times A \text{ category} + w_2 \times \text{island} + w_3 \times \text{trigger})$

b. Topicalisation:
$wf = (w_4 \times N \text{ category} + w_5 \times \text{island} + w_6 \times \text{trigger})$.

In (6), w_1-w_6 represent six different weights. Note that the sole relation between each w x s combination in (6) is addition, and it can be termed an adding model. The adding model is a simple case of

Anderson's general model and other variants are possible. One alternative is an averaging model, where the w x s combinations are averaged. In the adding model the assumption is made that the w x s values for a stimulus remain constant regardless of what other stimuli it may be combined with. In the averaging model the weight of any one stimulus depends on all the others.

(6) allows hierarchies to differ in importance within and between rules. Thus the two rule functions may differ in that they assign a different importance to a given hierarchy and yet be of the same general form.

The use of Anderson's model requires a theoretically adequate scale of measurement for J. This requirement is the general case of measurement problem (i), noted above. Testing the application of his model is the same as measurement problem (iii): to test the hypothesised form of the rule function. Anderson claims that both measurement problems can be solved simultaneously using one test of goodness of fit. If the data satisfy the goodness of fit test, the scale for J and the form for the rule function are jointly validated. It is desirable that the data be cast in the form of a factorial experimental design, and the goodness of fit test involves certain predictions about the results of the application of analysis of variance to the design.

An appropriate experimental design can be produced on the basis of a set of sentences which vary along one hierarchy i.e. vary in s, and vary across rules i.e. vary in w. This results in a w x s, Row x Column Factorial design, as in Matrix 3. The theoretical entry in each cell of the design is:

(7) $$R_{ij} = w_{ri} s_{cj},$$

where R_{ij} is the cell in the ith row and the jth column, w_{ri} is the weight associated with row i and s_{cj} is the value of the stimulus in column j. The assumptions required by

STIMULUS

	C1	C2
R_1	$w_{R1} s_{c1}$	$w_{R1} s_{c2}$
R_2	$w_{R2} s_{c1}$	$w_{R2} s_{c2}$

Matrix 3

equation (7) are that each weight is constant across its row and each stimulus is constant down its column.

Applying the analysis of rules given in (6), the first cell of Matrix 2 becomes: w_1 x A category + w_2 x B1 + w_3 x trigger. Since the w_1 x A category and w_3 x trigger values are held constant across

the row, and since they are added to the total sum, they can be represented by a single additive constant, K1, for the sake of simplicity. For the second row, w_4 x N category and w_6 x trigger can similarly be represented by a single additive constant, K2. Matrix 2 can now be rewritten as Matrix 4. The cell entries

		ISLAND	HIERARCHY
RULE WEIGHT	Adverb-preposing	$W_2B1 + K1$	$W_2B2 + K1$
	Topicalisation	$W_5B1 + K2$	$W_5B2 + K2$

Matrix 4

for Matrix 4 differ in form from the cell entries for Matrix 3 simply by the presence of the row constants K1 and K2. Since the addition of the row constants does not affect the goodness of fit test, Matrix 4, which represents the data of 3(a-d), can be considered as a w x s layout, and it is therefore clear that the data of 3(a-d) can form the basis for an appropriate experimental design.

The analysis of variance provides the test of goodness of fit. Applied to a two-way design such as Matrix 4, analysis of variance will show whether there is any significant difference between the levels of the Row factor, i.e. whether the two rules involved have a different effect on the well-formedness of sentences, and whether there is any significant difference between the levels of the Column factor, i.e. whether the two points on the island hierarchy have a different effect on well-formedness. In a design with more columns, the means across each row can be plotted as profiles to show the relationship between the rows in graphic terms. In the usual applications of analysis of variance it is hoped that the rows will plot as parallel curves. In statistical terms, if the rows are not parallel, there will be interaction between the Row factor and the Column factor, and the analysis of variance can determine whether there is significant interaction between the two factors. For the w x s layout given above, the goodness of fit prediction is that there will be significant interaction, that the row curves will not be parallel. However, it is claimed that the interaction will show a certain regularity. It is possible to partition the interaction into components and it is predicted that the interaction will be concentrated in the Bilinear (Linear x Linear) component i.e. that component associated with Linear relations between each of the two factors and the dependent variable, well-formedness. This is tested by subtracting the Bilinear component from the total interaction and seeing whether the remainder is significant.

Thus far, the application of Anderson's approach has provided a model for rule functions, a goodness of fit test for measurement problems and an experimental design for sentence data. This analysis may now be put to the test.

III. Testing

To test the hypothesis that the adding form of the summated weighting model was an appropriate analysis of the rule functions for Adverb-preposing and Topicalisation, an experimental design based on Matrix 4 was used, with the difference that the column dimension was expanded from two points on the island hierarchy to ten points. The ten predicates on this dimension were: 1) possible 2) expect 3) think 4) say 5) know 6) realise 7) find out 8) be surprised 9) regret 10) mention. These are given in order from weakest island to strongest island, on the basis of Lakoff's claims, and should therefore be associated with a decreasing degree of well-formedness. Matrix 4, a 2 x 2 matrix, is thus replaced by a 2 x 10 matrix. For the first column of this matrix, sentences were constructed using the predicate 'possible', as shown in (8).

(8) a. Yesterday it is possible that Mark filed a complaint.
b. <u>Dogs</u> it is possible that Arnold hates.

Underlining indicates stress in 8b. Similar sentences were constructed for the remainder of the matrix, using the appropriate predicates. The lexical items other than the predicates were varied to make the task of judgement less monotonous. The matrix was a w x s layout with the columns representing different island predicates and the rows representing different rule weights associated with the island factor, and the goodness of fit predictions were that there would be significant interaction between the rule weight factor and the island factor, and that this interaction would be concentrated in the Bilinear component.

A similar layout was set up for the trigger hierarchy, with two rows for the rules of Adverb-preposing and Topicalisation, and ten columns for ten points on the trigger hierarchy. The ten predicates selected for the trigger hierarchy were the same as those for the island hierarchy. According to Lakoff, the ordering of these predicates on the trigger hierarchy should be identical to that on the island hierarchy, and they should be similarly related to a decreasing degree of well-formedness. The sentences for the first column of this matrix are given in (9).

(9) a. It is possible that yesterday Bill bought the book.
b. It is possible that <u>taxes</u> Alice resents.

This matrix is also a w x s layout and the goodness of fit test makes identical predictions. For the trigger data, Matrix 4 can be rewritten as Matrix 5:

		TRIGGER HIERARCHY	
		C1	C2
RULE WEIGHT	Adverb-preposing	$W_3C1 + K3$	$W_3C2 + K3$
	Topicalisation	$W_6C1 + K4$	$W_6C2 + K4$

Matrix 5

The salient differences between Matrix 4 and Matrix 5 are that the row weights and row constants are different.

It may be of interest that Lakoff finds in his speech that the well-formedness midpoint (about .5) is reached in the middle of the trigger dimension for both rules (at 'realise') and similarly in the island dimension for adverb-preposing, but not for topicalisation which only reaches midpoint at 'mention'.

96 subjects were chosen from students at the University of Wisconsin-Milwaukee. 64 were from freshman level linguistics classes, 32 were from freshman level anthropology classes. The two groups' results were combined after it was found that there was little difference between them. The subjects were considered to be representative of a single dialect area with additional homogeneity in age and education and the results could therefore be expected to show a fair degree of uniformity.

The two matrices result in 40 sentences (2 x 20). Each sentence was presented simultaneously in written and spoken form and 10 seconds was allowed for response. Each subject was instructed to rate the well-formedness of each sentence on a 10-point scale which had been anchored at each end by an example sentence, a rating of 1 meaning 'completely well-formed' and a rating of 10 meaning 'completely ill-formed'.[1] The 40 sentences were put into a randomised order. Half of each group received the sentences in that order; the other half received the reverse of that order.

Each matrix was analysed as a randomised blocks factorial design with repeated measures (cf. Kirk 1968 Pg. 237). Since each subject received all treatments combinations, subjects were taken as a factor in the analysis, which was a three-way univariate analysis of variance, treated as a mixed effects model because the subjects were representative of a larger population while the treatment effects were fixed. Since this was a repeated measures design, conservative F tests were applied using the Geisser-Greenhouse correction.

IV. Results

Results are given in Tables 1 and 2 and Figures 1 and 2. The tables give the columns in the order predicted by Lakoff; the figures give the columns in an order determined by ranking the column means for each matrix. Note that 1 indicates a high degree of well-formedness and 10 indicates a low degree of well-formedness.

Fig. 1 and Table 1, the island hierarchy results, suggest that there are differences in well-formedness between the effects of the two different rules, that differences on the island hierarchy are associated with differences in well-formedness, and that the profiles for the two rules are not parallel, so that there is likely to be a significant degree of interaction. These impressions are supported by the statistical results shown in Table 3, which show sig-

TRIGGER HIERARCHY

	1	2	3	4	5	6	7	8	9	10	Across
	Possible	Expect	Think	Say	Know	Realise	Find out	Surprised	Regret	Mention	Predicates
Adverb-preposing	2.96	2.09	2.29	2.01	2.04	2.54	1.98	2.88	2.85	2.25	2.39
Topicalisation	7.82	7.32	7.28	6.85	6.53	7.16	6.99	7.71	7.12	7.45	7.22
Across Rules	5.39	4.71	4.79	4.43	4.29	4.85	4.48	5.29	4.98	4.85	

Table 2 : Means for Trigger Hierarchy Judgements.

ISLAND HIERARCHY

	1	2	3	4	5	6	7	8	9	10	Across
	Possible	Expect	Think	Say	Know	Realise	Find out	Surprised	Regret	Mention	Predicates
Adverb-preposing	5.32	3.22	3.87	6.23	3.83	4.64	7.05	6.84	5.96	7.67	5.46
Topicalisation	7.92	8.09	7.65	8.06	7.42	7.94	7.52	7.41	8.06	8.06	7.81
Across Rules	6.62	5.66	5.75	7.15	5.63	6.29	7.29	7.13	7.01	7.87	

Table 1 : Means for Island Hierarchy Judgements.

Summary Analysis of Variance for Island Hierarchy	
Source	F-ratio
Rules	132.2*
Hierarchy	24.2*
Rules x Hierarchy	23.2*

Table 3.

Summary Analysis of Variance for Trigger Hierarchy	
Source	F-ratio
Rules	317.8*
Hierarchy	8.7*
Rules x Hierarchy	1.7

Table 4.

Note: For tables 3 and 4 all degrees of freedom for F-ratios reduce to 1/95 with Geisser-Greenhouse correction. Each source has its own error term with 95 degrees of freedom. Significant F-ratios are indicated by asterisks.

Interaction Components for Island Hierarchy		
Source	Mean Square	F-ratio
Rules x Hierarchy	1016.1	
Bilinear Component	887.4	119.1
Remainder	128.7	3.8

Table 5.

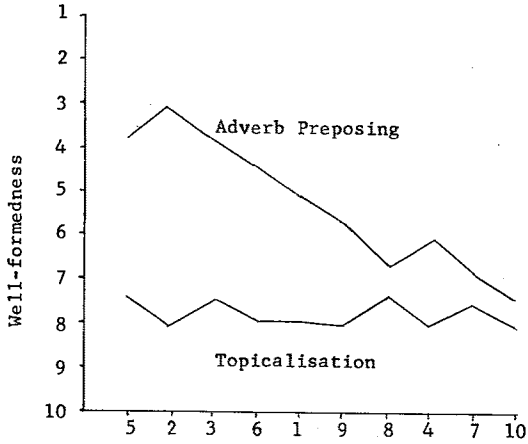

Figure 1.

Island Predicates

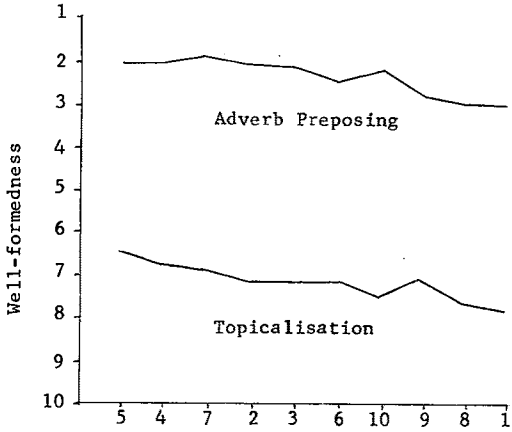

Figure 2.

Trigger Predicates

nificant F-ratios for the rule weight factor and the island hierarchy factor, and for the rule weight x island hierarchy interaction. Since the interaction is significant, the first prediction of the goodness of fit test is correct.

It remains to test whether the interaction is concentrated in the Bilinear component. To determine this, linear orthogonal coefficients were derived from the row and column means of the island hierarchy matrix as reordered in Fig. 1. The reordering of the means and the fact that there were only two levels of the rule factor weaken the power of this part of the testing. The partition of the interaction into a bilinear component and a remainder is given in Table 5. It can be seen that the F-ratio for the remainder is not significant, and so the island hierarchy data satisfy the goodness of fit test, with the qualifications mentioned above, and therefore support the hypothesis of the adding form of the summated weighting model for rule functions.

The trigger hierarchy results, given in Table 2 and Fig. 2, are similar in that they suggest that there are differences in well-formedness between the effects of the two rules and that differences on the trigger hierarchy are associated with differences in well-formedness. However the profiles for the two rules are not greatly different. The statistical results in Table 4 support these impressions: the F-ratios for the rule weight factor and for the trigger hierarchy factor are significant, but the rule weight x trigger hierarchy interaction is not significant. It would therefore appear that the trigger hierarchy data fail to satisfy the goodness of fit test. It is possible, though, to interpret these results in a way which does not disconfirm the summated weighting model. The rule weight x trigger hierarchy interaction could be expected to be non-significant if the two rules of Adverb-preposing and Topicalisation each assigned a similar weight to the trigger hierarchy factor. It does not seem unreasonable that two rather similar rules would give the same importance to one of their factors.

There were some discrepancies between the results for these subjects and Lakoff's findings for his own speech. The midpoints he found were generally not found here, and the Topicalisation-island hierarchy set of sentences, which were the most well-formed set for him were the least well-formed set in these results. More important were the discrepancies between the ordering of predicates he predicted and the ordering here. Note that the ordering of island hierarchy predicates was somewhat different from the ordering for the trigger hierarchy predicates. No obvious way of accounting for these differences suggests itself.

V. Conclusion

Lakoff has put forward a theory of fuzzy grammar in which the concept of rule function occupies a central place but is unspecified.

This constitutes a distinct weakness in the theory. I have proposed a simple but flexible model for rule functions which makes the theory more specific and which lends itself to valid and precise empirical test, thus subjecting claims in this theory to detailed empirical constraints. Further, in what is to my knowledge the first quantitative attempt to test the theory, the model was applied to judgments on a large number of sentences by a large, relatively homogeneous sample of subjects.

The results for the island hierarchy data and the trigger hierarchy data provide some support for the hypothesis that the adding form of the summated weighting model is an appropriate analysis of the rule functions for Adverb-preposing and Topicalisation i.e. the hypothesis that (6) is a correct representation of these rules. The evidence is by no means conclusive, but since this is the first exploratory study of the application of the model to sentence data, it is sufficient to establish the model as a tenable working hypothesis.

Anderson's approach lends itself to greater quantitative refinement than has been attempted in this study. If Anderson is correct, it would be possible to derive equal-interval scales for hierarchies and for rule weights, and to determine whether the more complex averaging form of the model is more appropriate than the adding model presented here, given further studies. This would lead to a detailed quantification of grammatical rules, at least for the individual speaker. But the present results do not seem sufficiently clear to support this degree of sophistication. It would seem to be a better strategy for the present to make general tests of the model over a range of rules, seeking to establish hierarchies more securely and to improve the methodology of testing sentence judgements.

It may be asked whether this model will apply to all rule functions. The present data cannot be used as a basis for such a generalisation. However, one feature of the model is that the rule function does not alter the rank order of the points within a hierarchy. The assumption that the rank order of a hierarchy is constant across rules is made by Ross, but is disputed by Lakoff (Lakoff 1973 pg. 282) who claims to have found a counterexample. It would certainly make it easier to recognise hierarchies if this assumption holds and it remains to be seen whether empirical testing of further rules supports it.

NOTES

* I would like to acknowledge the assistance of the University of Wisconsin-Milwaukee in providing the resources for this study. Thanks are due to Marshall Dermer, Sidney Greenbaum, Robert Remstad and Walter Zwirner for stimulating discussion and to Donnie Dean, who gave much thoughtful help.

[1]The actual instructions were:

"Consider each of the sentences and decide if it would be possible that you would say this in conversation. An example of an obviously possible sentence is:

"John plays golf"

It is clearly normal and natural. An example of an obviously impossible sentence is:

"John will chasing me"

It is clearly abnormal and unnatural."

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III

Linguistics as Wissenschaftspolitik

REVELATION AS LINGUISTIC REVOLUTION

Raimo Anttila, UCLA

"Chomskyan revolution" is a notion that developed during the past decade, and it became an almost household word in neighboring sciences also, and not only in "mainstream" linguistics. Quite recently, however, an increasing number of linguists have realized that this allegedly linguistic revolution was a social coup d'état. This conclusion comes out through diverse channels, e.g. the open letters of 1974 to the LSA by Ernst Pulgram and J. Peter Maher, Adam Makkai's comment to George Lakoff at the 25th Georgetown Roundtable Meeting, March 15, 1974, as well as various other publications, e.g. Gray (1974), Adam Makkai (1973, 1974), Maher (1973ab, 1974), and Anttila (1974abc). But as fashion would have it, this revelation (as revolution) of new linguistics became "modern and theoretical linguistics" par excellence. Adam Makkai's contribution to this volume brings out admirably the salesmanship employed in such successful marketing.[1] The situation is most interesting from the sociological and historical point of view,[2] and my observations here are meant as a plea to record and study recent history when much of it is still easily recoverable.

Revelatory revolutions tend to be ongoing in that the very persons who initially prevent common sense later make progress by adopting the views they earlier rejected so vehemently. Peter Maher has drawn our attention to this manoeuvre by comparing recent linguistic behavior with the Oxford linguistic philosophy as analyzed by Ernst Gellner (1974). As particular linguistic examples we have Robin Lakoff's article "Language in context" (1972) and Paul Postal's statement (Amherst, summer 1974) that there is no necessity to reject transformational grammar, since such nonsense never existed. In their respective work both were champions against the line now adopted. In this vein many who would regard themselves as some kind of neotransformationalists defend early Chomsky by maintaining that he himself never intended such a blatant takeover and that he is not responsible for the actions of his followers. As a recent presidential precedent has shown, this is a shallow argument, since there should be scientific responsibility when you hold a position of power. Another recent progress statement goes that "Chomsky is now favorably inclined toward Bever and Langendoen's experiments". This is supposed to solve the original innatism and the cul-de-sac of the past decade. Be it remembered that Chomsky originally "hate[d] experiments" (Mehta 1971), which soon reflected into all of "modern" linguistics and enhanced the charm of "interesting" speculation. But an argument through "is NOW favorably inclined" implies that all progress should come from a few chosen linguists who are allowed to erase history as they progress. This progress is powerful in that it immediately makes earlier follies inoperative and prevents criticism by adopting this very criticism as a new breakthrough of one's own model. The social meaning of this is that the linguists who always stuck to the now correct position are branded old-fashioned or the like; since they were ahead of their times, they are not "modern". Only "modern" linguists deserve credit, and not those whose efforts were premature (as Chomsky said about Jespersen and Sapir at Amherst 1974).

The stance I have described asks that history should be written from a narrow and partial point of view, or that if an offender confesses, there was no offence at all. On the contrary, we must use every bit of evidence

available to study the recent history of linguistics. Reliance on the traditional material of published reports does not help us now, since private action and public discussion are so much at variance. The more time will elapse, the more personal experience will be forgotten, and more evidence will be lost. To reject non-written or unpublished evidence would mean condoning tape erasure of history; and admitting it to bear upon the issues at hand is not only important supplementary evidence for the recent history of linguistics, but can also be crucial for the understanding of the sociological phenomena involved.

A difficulty is in where to draw the line between confidentiality and public statement. Tête-à-tête conversations fall within the former domain and oral statements before audiences within the latter, although there can be intermediate situations. If one wants to maintain that privately expressed sentiment with the exact sources suppressed cannot be history, one is still left with valid sociological inquiry into the beliefs and opinions held within a particular group. This is indeed an area where recent history and sociology meet, and as was mentioned, this should be carefully studied. Use of "private" opinion has a twofold effect: (1) Taking resort to it may sound like hearsay (which can be interpreted either positively or negatively depending on your own predilections), or (2) it gives a perfect defensive shield for those whose ideas come under criticism, since they can brush off adversaries with a "Whoever said that?". This is the manoeuvre brought to our attention by Maher (1974), who refers to Gellner's analysis of the Narodniks of North Oxford. Because the situation is as it is, linguists have to collate personal experiences. Note also the fact that so much of current criticim is directed against transformational-generative grammar and its various offshoots. It does not mean that an innocent is brutally drawn under gang attack, but that they are indeed real offenders who have brought scholarly discourse to an all-time low. That they are the central targets is of their own making, and furthermore, they should have scientifically justified their position in the first place. This was not done; revelation was enough.

The revelatory nature of transformational grammar comes out best in the propagandist use of the terms universal, general, interesting, and clear. Any alternative solutions or possibilities are labeled as not these. Furthermore, linguistics is said to be a mentalistic discipline, part of rationalistic thought. The rationalism is of the Cartesian type (clear and distinct to oneself only, largely through revelation), and it is used to ward off any contrary evidence. Thus the level of discourse is beyond this world. Still, language and linguistics are said to be empirical in some unmentionable sense. (For true rationalism and empiricism see Uhlan Slagle's contribution to this volume.) Bennison Gray points out many other similar contradictions which are typical of revelation passing as science. But even the mentalistic label is unfounded, since transformational grammar believes that linguistics is a natural science in the neopositivist sense. Rather, linguistics is a true mentalistic science (Geisteswissenschaft), whereas transformational grammar remains perfectly in the behavioristic mould (Itkonen 1974), propagandist announcements to the contrary notwithstanding. Such contradictions abound in the (TG) theory, and when a theory is full of them, we get enhancement for the built-in success mechanism pointed out by Gray (5a). When criticism hits any point, one always has an opposite extreme to take to for defence.

Also the term formal tends to be identified with logic, or something very desireable. Gray's suspicion that TG "logic" might not be logic at all (4a), but at best techno-logic, is correct. Command logic is not logic. In

any case, Gray realizes that formal logic is irrelevant (10b), and indeed, the issue with formal languages misses the very essence of natural language. Charles Ruhl gives an elegant statement of this in this volume by showing that the academics abolished the distinction between formal and natural language. Linguists had too flat and mechanistic a view, and this led to over-analysis. Of course there is form also, but from the semiotic point of view that is the "lowest" level. Furthermore, such form is substance- or medium-oriented (mittelbezogen; Walther 1974:125). The kind of transformational sentence analysis (generating syntactic trees from S downwards) is actually known as degenerative semiosis. This is a technical semiotic term (Walther 100), to be distinguished from other degenerative aspects of formalization (Anttila 1974b). Again, Gray's statement that a grammar cannot be generative (3b), or that "generative" is an empty metaphor (10a), gets further support. Naming things at will, but mainly by the opposites, does indeed create an Alice in Wonderland effect (Gray 5b). This is typical in social revolutions (e.g. calling state capitalism socialism).

The 1970s have now shown a distinct manoeuvre by transformational-generative grammar back into traditional notions, with varying degrees of success (Anttila 1974a). Basically this is done by pointing out that the study of formal systems has now progressed far enough to return to the old functional aspects. The progress that results is a distortion of both history and conceptual analysis, since function is the primary side. At this conference Slagle has often mentioned the pointlessness of unmotivated formalism, on which most of transformational-generative grammar was built. But the overly confident reliance on formalism is still a strong force. Particularly revealing is the following ploy, which shows the use of the built-in success mechanism and the fact that making progress is reserved for a chosen few. Criticism of the early ahistorical purely formal (and hence taxonomic) generative historical linguistics is answered by a statement that one actually meant exactly what is now found lacking in the approach (Anttila 1974b). What is at issue here is a very powerful defense mechanism, since it is tantamount to defining a private language post facto. Certainly community language was not used, if this is the case. On the other hand, this is another TG vacuity, because private languages are logically impossible. Such contradictions can only exist in blind revelation.

The strong personality cult seen e.g. in reserving all steps of progress for a chosen few takes on various other forms. First there is private argument ad hominem. This can take two forms. In 1973 a representative of generative grammar did not admit to me that the original generative position on historical linguistics was wrong, but would do so to another linguist a few days later. Or else criticism based purely on scholarly issues is taken as a personal attack (as has happened to Anttila 1974c). This kind of imaginary personal attack can now be taken as very indecorous behavior. But such arguments can also occur in print, and they can take two forms. Frances Karttunen (1974) resorts to mere character denigration omitting scholarship altogether (and she does this in a scholarly journal). Françoise Bader (1972), on the other hand, finds it sacrilegious to question the ideas of the grand master (Benveniste), and that in general it is bad manners to point out flaws in the earlier work by masters. Of course there are flaws and mistakes in anybody's work, but the above attitude misses the point by expressly defending personality cult. There are current sociological parallels for going berserk over the human factors in science. Thus (leftist) university innovators (in Europe) maintain that since it is impossible to keep politics totally out of

science, science should be maximally politicized. The conclusion is misguided and wrong. Similarly one should try to minimize personality factors or their adverse effects in science. This requirement does not deny the fact that science is of course carried out by people and not by identical machines, although transformational-generative training has come close to producing such machines (Anttila 1974b).

Generative-transformational colonialization of linguistics imposed thus quite definite constraints on who can say what and when. To be listened to one had to profess the right group allegiance. My defense of traditional analogy against generative distortion was expressly ignored because I did not belong to the right ingroup, or any group. Two linguists have now told me that I should have joined (or infiltrated) such a group (as they partially did) in order to have tackled the issue from within. Such advice bears witness to the curious sociological aspect in modern linguistics (although such a situation has not been unknown earlier either). We are back at where we started out from: predestined progress. Peter Maher's squib to Linguistic Inquiry, which tried to correct gross misrepresentation in Sound Pattern of English, was returned with "It is easy to find mistakes".

The salesmanship among transformational grammar was able to establish a general equation of transformational and theoretical, another of these unfounded labels. Adversaries were derided as compilers of data and treaters of mere method or strategy. Such a dichotomy was unfounded since the pragmatic requirements necessitate a basic sameness of theory and method. Recently (1973) Ferenc Kiefer and Robert King have justified generative grammar for the exact training they got within it (see Anttila 1974a). Such honesty is extremely rare on the current scene. They say that they would not be the linguists they are without generative training. Here one might easily forget that other forms of structuralism have also provided good training to many linguists. Again we have a total shift from one end of the scale to the other. The danger is in that the "old" terms remain, making Tarzan thinking (to use Hockett's term) possible.

The ingroup mentality is still strong and by no means a matter of history. In a recent discussion with Peter Maher Zwicky (1973) not only requires that books be reviewed by insiders or sympathizers, but also that only transformational grammarians be allowed to use humor and wit, and not their critics (Maher 1973b). At the same time a proponent of generative phonology says that McCawley intended his generative phonology as a joke to see how soon his benighted colleagues would see through it. A similar opinion has been expressed by another linguist concerning Lightner's work. As it is, the jobs certainly went to the jokesters. Furthermore, it is very curious that the same ingroup has been so concerned about social justice in general. But inconsistency is the consistency in this school. Note also Chomsky's criticism of the Pentagon boys (see Maher in this volume). The same criticism applies to Chomsky's own Mandarins, but in the former case the behavior discussed is considered bad, in the latter case a model of scientific inquiry!

Inconsistency leads also to a new form of variation which could be called "linguistic at-variance". In 1958 Chomsky addressed a group of engineers at Ann Arbor telling them that he had applied the computer model to develop a theory of language. A decade later he cosigned a statement that computer technology never had anything to do with transformational grammar. On July 26, 1974 (at Amherst, in response to the Collitz lecture), Morris Halle stated that he heard for the first time about Esa Itkonen. This sounds curious in the light that Itkonen had spent a whole year at MIT studying linguistics. Does this mean that Halle wanted to convey the idea not to pay atten-

tion to Itkonen's work? In any case it all shows the necessity for not believing blindly in what is said or written, even in supposedly scholarly contexts. We need a broader basis for studying the history of linguistics.

Finally, a short mention should be made of language as Geisteswissenschaft (Itkonen 1974). All cultural sciences need a hermeneutic method of understanding, not the explanation of natural sciences. Hermeneutics means interpretation, and in any kind of communication signs must be interpreted. Transformational grammar, however, denied language use by concentrating on mere tools without use, and also Walther (100) points out that the school is not adequate from a comprehensive semiotic point of view. In any communication interpretation and interpretants is the most important axis. Such a broad view of semiotics seems to be the very program that Victor Yngve asks for in his human linguistics in this volume. As tagmemics has been discriminated against because its practitioners are interested in religion also, so do transformationalists and other neopositivists connect hermeneutics with Marxism and Maoism. In fact, some of the hermeneuticians themselves may make such an unwarranted claim. This is pure propaganda on both sides. As came out in connection with Adam Makkai's paper at this conference, political systems have nothing to do with the description of language. Theoretical attitudes can be packaged and sold many ways.

Thus the dangers of revelation lurk everywhere. In my 1974 Collitz lecture I discussed August Boeckh's program of hermeneutics (for a reference see Anttila 1974d), and it is obvious that Boeckh did not grind any political axes. Many philosophers of science could be cited in support of the same position, but I want to refer to an anthropological linguist, Richard Conrad Schmidt (1974). His article appeared right at the time of this conference. He points out that phenomenology is a method, exactly like structuralism, and this was also Köhler's view (see Slagle's contribution for references). It is true that Schmidt does not specifically mention hermeneutics, but he does refer to Schutz and Verstehen, Kant and Bewusstsein, or appreciation rather than traditional explanation (25a). Note also that Itkonen reaches the hermeneutic position through Continental philosophy, Schmidt through anthropological linguistics and a line of American philosophy. As Dell Hymes and Robert Austerlitz have been pointing out, anthropological linguistics is synchronic philology, and thus it is no wonder that this development meets Boeckh's philosophical philology, which many historical linguists have been following, although often quite implicitly.

As Adam Makkai said in the opening session of this conference, there is a lot of nonsense to be weeded out of linguistics, and without hounding down those who had been misguided. But some of this nonsense must be remembered and codified for a maximally accurate history of linguistics. For this we need sociological field methods also, or a component of oral history.

NOTES

1. This inaugural LACVS forum turned out to have much support for my theme. I have written this version accordingly, with my thanks to the participants and the discussion that does not appear in the proceedings. Particularly relevant for my message are the papers by Maher, the Makkais, Ruhl, and Slagle (note also Lamb's contribution for a combination of various TG inadequacies under one principle). These papers are not listed in my bibliography below, which is also otherwise fragmentary, due to certain unfortunate circum-

stances. There is a rising body of similar evidence, all of which is not considered here, e.g. Ohala (1974), also "from Chicago".

2. Peter Maher has for quite some time called for a sociological investigation of what linguists do and how they do it.

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A VIABLE ALTERNATIVE TO CHOMSKYAN RATIONALISM

Uhlan V. Slagle

Arlington, Virginia

The 'Cartesian' rationalism of Chomskyan linguistics represents a fundamental misunderstanding of the nature of language and mind. Whatever plausibility Chomsky's counterintuitive theory may have rests on the totally mistaken assumption that empiricism can offer no viable alternatives. In this study we will show the complete invalidity of such an assumption. Indeed, it will be demonstrated that a far more powerful and coherent explanation of the nature of language and mind is possible within the context of empiricism than within the framework of Chomskyan rationalism. The key to an adequate empiricist theory lies in a synthesis of the relevant insights of Kant (which will surprise many), Mill, and Gestalt psychology. Such an assertion may seem questionable at best to many scholars, given the many dubious, and often conflicting, arguments of Kant and Mill, and given the fact that the neurophysiological hypothesis generally advanced by the Gestalt psychologists is demonstrably wrong in many respects. Nevertheless, the aforementioned have made invaluable contributions toward understanding the nature of language and mind. Kant and Mill have provided the key to correlating the structure of logical meaning with the structure of experienced reality, while certain indisputable insights and experimental findings advanced in the work of the Gestalt psychologists furnish the basis for showing that the immanent organizational factors underlying the dynamics of thought are already operative in the organization of perceptual experience (perception itself being a dynamic process). As a result, by examining the aforementioned contributions of Kant and Mill in the light of the relevant insights and experimental findings of Gestalt psychology, we will be able to show the underlying unity of the perceptual and cognitive processes, and thereby establish a theoretical framework for correlating the structure of perception with the structure of thought and both logical and lexical meaning. In maintaining that it is possible to correlate the structure of perception with the structure of thought and meaning, we shall go considerably beyond the claims of Chomskyan rationalism. Moreover, in showing the underlying unity of perception and thought, we will be able to provide a far more unified and coherent account than Chomsky has offered of the relationship of syntax to semantics, while also furnishing a universally valid foundation for comparing and contrasting how different languages segment reality.

In the process of presenting our theory, we will be, in effect, systematically showing that, whatever the currently prevailing authorities may contend, rationalist theory can derive no really valid support from either recent split-brain research or the epistemological implications of 'modern' mathematics and physics. At the same

time, we will be demonstrating that the theory being advanced in this study provides a viable solution to the complex set of psychological and epistemological considerations which any contemporary theorist must face if he is to adequately deal with the problem of cognition in general, a condition which must be met if linguistics is ever to be adequate to its subject matter.

Having oriented the reader in regard to our goals, we can now begin the actual presentation of our theory. We will start with an examination of the nature and significance of the contribution made by the Gestalt psychologists.

The real contribution of the Gestalt psychologists lies in their discovery of a previously unknown dimension of the perceptual process. This is the discovery which provides the key to overcoming the difficulties which have traditionally beset attempts to understand the cognitive processes and their relationship to perception. The Gestalt psychologists were able to show that similarity is a factor underlying the spontaneous unification of discrete stimuli presented to the perceiver.[1] In other words, similarity has been shown to play a major role in determining which stimuli are spontaneously unified in the process of perception. Now it is extremely important to note that the spontaneous unification effected by similarity functioning in its role as a principle of spontaneous grouping represents a mode of perceptual organization that is not present in the stimulus pattern itself. Perhaps the most dramatic proof of the role of similarity in effecting spontaneous perceptual unification comes from studies of stroboscopic movement.[2] Thus, for example, given two lights arranged in a row, set a few inches apart, if we switch the left light on and off, and then at the correct temporal interval switch the right light on and off, the light on the left will <u>appear</u> to move across the intervening space to the right even though there is <u>no</u> movement in the stimulus pattern itself. Now, if we conduct the same experiment using three lights set at equidistant intervals and switch the center light on and off, and then simultaneously switch on and off the lights on both sides, we can predict on the basis of similarity in which direction the light in the center will <u>appear</u> to move (cf. Krech 1969:223). If, for example, the light on the left is similar to the one in the center, then we will normally experience movement to the left. And if we were to put the similar light on the right, then the center light would appear to move across the intervening space to the right, with similarity once again playing a major role in determining what we see. An even more impressive illustration of spontaneous unification in sensory fields can be brought about by the short exposure of an upside down V-shaped figure, followed, at the proper interval, by the exposure of an upright V-shaped figure situated beneath the first figure. In this case, where once again no movement exists in the stimulus pattern, we will tend to perceive a single figure flipping over through the third dimension. This means that what really is the successive exposure of <u>two</u> figures without any movement is perceived as <u>one</u> figure flipping over through a dimension not given in the stimulus pattern (cf. Krech 1969:223). In yet another experiment it can be demonstrated that given two columns of lights, each consisting of two lights, if one simultaneously switches on and off the top light of one column and the bottom light of the other column,

and then switches on and off the remaining lights at the same time, one will perceive movement in opposite directions simultaneously, with an upward movement being seen in one column and a downward movement being perceived in the other column at the very same time (cf. Krech 1969:221). (Here once again there is no movement in the stimulus pattern itself.) The last experiment shows something extremely important: namely, that one cannot explain away the Gestalt psychologists' findings in regard to spontaneous unification through traditional theories--for none of the traditional theories can explain the basis of perceiving stroboscopic movement in opposite directions simultaneously.

Spontaneous sensory unification also takes place in static perceptual experiences. The famous Wertheimer dots, for example, convincingly illustrate the role of similarity in determining how a stimulus pattern is perceived. And we would emphasize that none of the traditional theories can explain the role which similarity plays in determining how we perceive a field of Wertheimer dots. Here a word of warning must be issued in regard to the adaptations of the Wertheimer dots found in many texts. For if the qualitative contrast between the two sets of dots is not strong enough, and it is not in many of the adaptations, then the role of similarity is far less convincingly illustrated than in the original examples.[3] In addition to the Wertheimer dots, there are a number of other illustrations of the fact that even in static experiences the perceiving organism often imposes an organization not given in the stimulus pattern itself, perhaps the most famous of these being the well-known Müller-Lyer illusion, which convincingly demonstrates that perceived size is strongly influenced by the larger perceptual whole in which a given perceptual object is perceived.[4]

There is then no doubt that our sensory faculties often impose a mode of organization not given in the stimulus pattern itself. Nor in this context is there any doubt that similarity plays a central role in effecting the spontaneous unification of stimuli in perception. The significance of this finding in regard to the role of similarity is brought into perspective when one realizes that since the time of Aristotle (cf. Süllwold 1964:36) similarity has been regarded as a factor influencing the recall of past experience; and when one further realizes that William James (1950:vol.2,641-51), William Stanley Jevons (1890:121-23), John Stuart Mill (see Slagle 1974b), Walter Ehrenstein (1965:170-218), and many others have persuasively argued that similarity underlies logic, with respect to both inference and categorization. Thus, the factor which the Gestalt psychologists have shown to effect spontaneous unification in perception, has traditionally been considered to underlie both memory and thought. This, of course, strongly suggests that there is an underlying unity of memory, thought, and perception. And it further indicates that one can explain both recall and categorization in terms of the functioning of similarity as an immanent organizational factor of sensory experience. This is, as we shall show, actually the case.

For as Köhler and Restorff 1935 suggested, similarity functioning as a principle of spontaneous unification underlies the process of recognition by effecting a functional union of trace process (i.e., memory) and perceptual process. The validity of this posi-

tion is made clear when one examines such phenomena as Gestalt completion figures. For they, as Wallach 1961 correctly observed, prove that memory processes can become functionally united with perceptual processes, with the memory 'traces' actually supplying the missing aspects of incomplete figures in such a way as to unify a number of seemingly disparate elements into a coherent whole. Thus, for example, when this functional unification takes place, what often at first appears to be merely a group of unconnected and unrelated elements in the visual field, spontaneously reorganizes itself, with the elements literally snapping into place, and the previously apparently unrelated elements being perceived as parts of a unified whole representing something one has seen in the past, such as, for instance, an airplane or a ship.[5] Now it is perfectly clear that memory can furnish the missing aspects of Gestalt completion figures only if the process underlying the memory of a given experience is highly similar to the process underlying the perception of that phenomenon.[6] Therefore, since we know that similarity effects spontaneous unification in sensory fields, and since we must posit a high degree of similarity between the perceptual process and the memory process in order to explain Gestalt completion phenomena, we can safely assume that similarity functioning as a principle of spontaneous unification in sensory fields underlies our ability to recognize just what the Gestalt completion figures represent. Note that our memory can complete the incomplete Gestalt even when the Gestalt completion figure differs from any previous experience of the phenomenon in question in regard to size and specific detail. For what memory actually supplies is the underlying pattern and this is what causes the unconnected elements to be seen as a unified recognizable whole. Now the more fully the incomplete figure is depicted, the easier it is to recognize. Consequently it is apparent that the functional unification of perceptual process and memory ('trace') process is effected far more readily under normal stimulus conditions when the stimulus pattern is complete. Thus, it is clear that similarity, functioning as an immanent organizational factor of sensory fields, underlies the process of recognition and consequently the process of categorization. This means that while categorization is, as the rationalists argue, a rule governed process, it is nevertheless a process which is totally explicable in terms of the immanent dynamics of sensory fields. There is, then, an underlying unity of thought and perception, a unity which is fully explicable in terms of the structure and immanent organizational factors of sensory experience.[7] And so it is that one can explain the integration of the arbitrary symbols of language into the memory network in terms of the role of contiguity. For contiguity also has been shown beyond any doubt to function as a principle of spontaneous grouping in sensory fields (cf. Wertheimer 1923, Köhler 1947). Therefore, the conjoining of symbol and experience exemplified in ostensive definition is enough to establish a functional union between an arbitrary symbol and the experience it designates. Consequently, after this union is effected, any future experience of that symbol will tend to activate the memory of the appropriate phenomenon, while future experiences of that phenomenon will in turn tend to activate the memory of the appropriate symbol. This means that by manipulating the arbitrary symbols of language, we can selectively

activate the processes underlying the memory of any given data. Such manipulation brings with it considerably greater freedom. For by manipulating these arbitrary symbols we can effect conceptual constructions which as constructions need not be copies of what is given in immediate perceptual experience, even though their ultimate constituents are necessarily derived from immediate sensory experience. Thus, even man's ability to form concepts of imaginary entities is fully explicable in terms of the functioning of immanent organizational factors of sensory experience.

Many, of course, will think that the findings of recent split-brain research invalidate the empiricist theory we are advancing. This assumption, however, is clearly wrong. For while split-brain research shows that there is certainly a considerable degree of hemispheric specialization, it also shows that subjects, using only the language dominant hemisphere, can recognize rather complex Gestalten.[8] And recognizing these figures requires that the subjects not only recognize the given distinctive features but also the systematic arrangement of those distinctive features. Otherwise, it would be impossible to distinguish between any given random arrangement of those distinctive features and a patterned arrangement representing the given phenomenon. Clearly, the subjects are able to make this distinction. In this context, consider for a moment the stick figures used to entertain children, where using the same set of geometric features, one can make a number of recognizable figures by simply putting the same features together in different patterns (i.e., Gestalten). What has given the 'rationalist' interpretation of split-brain research a certain plausibility is the fact that the language dominant hemisphere seems a bit lacking in the 'artistic' sphere. Once again, however, they are on the wrong track. This is proven by the fact that most children, and many adults, can draw little accurately from memory (or otherwise), yet are able to recognize immediately, when confronted with an accurate depiction of the phenomenon in question such as a photograph, that their drawing is incorrect and that the photograph is accurate. A memory trace (of whatever nature) must underlie the ability to make this distinction between the inaccuracy of the drawing from memory and the accuracy of the photograph. And since memory indicates that the photograph is correct, the way information is stored in the mind must more closely resemble the photograph than the drawing. Consequently, there is no valid basis for believing that one's drawings from memory accurately indicate how information is stored in the mind. A similar line of reasoning applies to counterarguments based on the unreliability of mental images, since the unreliability, or even the total absence of mental images, does not interfere with our ability to distinguish, without any repetition of the original experience, between the accuracy of inaccurate drawings from memory and a photograph or accurate drawing. In this context it is illuminating to consider the fact that while a high degree of similarity between the perceptual process and the memory trace process is a necessary precondition of some Gestalt completion phenomena, we need not be able to visualize the object in question in order for our nervous system to effect the functional unification of trace process and perceptual process which takes place when the seemingly unrelated elements spontaneously snap into place and a unified fig-

ure representing something familiar is perceived for the first time. Often simply being told what the incomplete figure represents is enough to set off this spontaneous rule governed unification of memory and perception. In such cases, hearing the correct label is enough to activate the appropriate memory trace so that it can complete the incomplete figure by supplying the underlying pattern. And this can happen without our having any mental image of the object represented by the Gestalt completion figure. Thus, in the light of the foregoing considerations, it is clear that neither the recent arguments based on split-brain research, nor those based on the unreliability (or absence) of mental images provide a valid foundation for criticizing the viewpoint advanced in this study. It would appear, then, that we are on solid ground in suggesting that one can explain the dynamics and structure of thought in terms of the dynamics and structure of sensory organization.

Having established a theoretical framework for showing the unity of perception and thought, we can now profitably turn our attention to Kant's contribution to understanding the nature of logic. As Kemp Smith (1962:36) astutely observed, the "demonstration of the imperative need for a complete revisal of the whole body of logical science, is the first, and in many respects the chief, outcome of his (i.e., Kant's) Critical enquires." Kant's breakthrough came in establishing a systematic correlation between the structure of sensory experience and the innate categories of thought. He did this in attempting to explain the basis of the applicability of logic to sense experience (cf. Milmed 1961:26). For Kant's transcendental schemata represent referential rules for the Kantian categories (cf. Körner 1955:70-73;Beck 1969:480). And these schemata, or referential rules are based on the modes of the possible temporal ordering of sense experience (cf. Wolff 1963:222-23). Within his theory, however, not only the recognition of the temporal organization of sense experience is necessary in order to apply the schemata but also the recognition of the spatial ordering of sense experience is necessary (B291-93). According to Kant, the function of the principles of understanding is to give us information about the systematic interconnectedness of our possible perceptions, insofar as we are able to determine in advance of any particular occasion what any given perceiver would experience under any given set of circumstances (cf. Lindsay 1934:134-35). Kant correctly realized that atomistic associationism cannot explain the ability to recognize the difference between the subjective order of our personal experience and the objective order of things as they represent a system of constant and systematic interconnections of possible experiences valid for any given perceiver (cf. Lindsay 1934:109). For what we experience on any given occasion is often misleading. Thus, for example, we often think at first that there is a causal relationship between two events only to discover in the course of time that there is none. Only little by little, then, do we learn to recognize the constant relations holding between different sets of possible sensory experiences. Hence, although experience provides the data, the capacity for organizing sense experience into a unified body of knowledge requires the ability to systematically relate present experience to past experience on the basis of perceived similarity, and this ability to systematically relate (i.e., to classify) is obviously not

derived from experience but is innate. According to Kant, it is, of course, the innate categories of logic which underlie our ability to relate on a rule governed basis what we perceive on any given occasion to the 'total' system of constantly recurring spatio-temporal connections between the sets of possible perceptual experiences which constitute objective reality as it is given to our senses (cf. B244-46,313-14). Consequently, as we have already indicated, the transcendental schemata, or referential rules for the categories, can be applied only if the specified patterns of spatio-temporal organization exist between the given sets of possible perceptual experiences. Thus, the categories of Kant's transcendental logic represent rules of spatio-temporal ordering. Within the context of the psychology of Kant's time, it was, of course, impossible to explain the rules underlying categorization in terms of any sensory mechanism. Today, however, we are able to explain categorization fully in terms of the factors underlying the dynamics of sensory organization; in fact, as we have already shown, categorization is effected by the functional unification of memory trace process and perceptual process brought about by the functioning of similarity as a principle of spontaneous grouping in sensory fields. This applies to recognizing the recurring sames of the spatio-temporal organization of sense experience, just as it does to recognizing the recurring sames represented by simpler perceptual data. Moreover, the fact that one can explain the process of categorization in terms of the functional unification of memory process and perceptual process effected by similarity functioning as a principle of spontaneous unification in sensory fields, shows that one can explain the rule governed functional unification of perception and memory in terms of the functioning of an immanent organizational factor of sensory experience. And it is, of course, this rule governed functional unification of memory and perception which underlies our ability to organize sense experience into a unified whole. Thus, the functions Kant attributed to the Kantian categories can be explained in terms of the functioning of immanent organizational factors of sensory experience. These categories, it must be remembered, underlie, according to Kant, not only the referential rules represented by the transcendental schemata, but also the logical form of analytic thought which constitutes the domain of traditional logic (this a point which is often conveniently overlooked)(cf. B105). Consequently, the fact that we can explain the functions of the categories in terms of the functioning of immanent organizational factors of sensory experience, provides additional powerful support for our contention that there is a thoroughgoing unity of the perceptual and cognitive processes. Moreover, our ability to explain the categories in this way, strongly indicates that logical meaning has as solid a foundation in sense experience as lexical meaning has, with logical meaning being based on the higher level invariants of sense experience such as the recurring patterns of spatio-temporal ordering. The validity of this position will become rather clear in the course of our examination of Mill's logic. For Mill fully realized that logical meaning is based on the patterning of sense experience (cf. Slagle 1974b). Just what is meant by the phrase "patterning of sense experience" in this context is brought into sharp focus by an examination of the meaning of the syllogism. Consider, for example,

the affirmative syllogism

> All men are mortal,
> The Duke of Wellington is a man,
> therefore
> The Duke of Wellington is mortal.

Mill explains the meaning of this syllogism in the following way: the first premise asserts that all denotata which manifest the criterial conditions for being labeled a man will also manifest those for being labeled mortal; consequently, given the fact that the Duke of Wellington fulfills the criterial conditions for being categorized as man, he necessarily also manifests the criterial attributes for being categorized as mortal, since we know from the first premise that the criterial sets of features required for correctly applying the term man always co-occur with those for applying the term mortal. According to Mill, negative syllogisms illustrate the same principle. Consider for instance the syllogism

> No men are omniscient,
> All kings are men,
> therefore
> No kings are omniscient.

This means simply that the set of criterial attributes denoted by the term man never co-occurs with those for omniscient; and since all kings exhibit the necessary criteria for being classified as men, they therefore cannot be classified as omniscient since the set of attributes required for omniscience never appears conjoined with those for man--a fact established in the first premise. Thus, in his System of Logic, Mill argues that both affirmative and negative syllogisms are based on the co-occurrence patterns of sense experience (cf. 2.2.3). Moreover, Mill explicitly states that the propositions of logic are ultimately based on sequence, causation (i.e., uniformities of succession), co-occurrence, existence, and resemblance (cf. 1.5.7). Surprisingly enough, these categories are all based on the psychological universals Mill recognized as underlying sensory organization (cf. Slagle 1974b). Within Mill's system of logic all of these categories relate to sense experience and its spatio-temporal ordering, with the recognition of resemblances underlying our ability to discern and categorize the recurring patterns of co-occurrence and succession.[9] This, of course, represents an empiricist parallel to Kant's transcendental logic. In both cases, logic is concerned with the spatio-temporal ordering of sense experience. Thus although he approached the topic from a much different viewpoint, Mill reached the same truth in his System of Logic that Kant had illuminated in his transcendental logic. And while it is true that Kant never realized the full implications of his transcendental logic in regard to traditional logic, it is equally true that he explicitly stated that the same categories of the faculty of understanding underlie both the forms of judgement of traditional logic and the transcendental schemata of his transcendental logic (cf. B105). Thus, it is a matter of considerable consequence that, as we have already shown, one can explain the functions Kant attrib-

uted to the categories in terms of the functioning of immanent organizational factors of sensory experience. In this context our examination of Mill's logic has further underscored the plausibility of our assumption that there is an underlying unity of the perceptual and cognitive processes.

From the foregoing considerations it is clear that one can explain the nature of logic in a unified and coherent way by explaining the dynamics and structure of thought in terms of the dynamics and structure of sensory organization (exemplified in the rule governed functional unification of trace process and perceptual process effected by the functioning of similarity as a principle of spontaneous unification in sensory fields). Within such a theory the so-called logical words, such as 'or', 'not', and 'if', simply give us information on the patterning of sense experience. Thus, for instance, the use of 'or' in "A baby is always a boy or a girl" simply indicates that any baby will exhibit one of two sets of sex characteristics, and that the two sets of sex characteristics will not co-occur in any given baby. The term 'not' is no more problematic than the term 'or'. For example, the term 'not' in "She is not thin" simply indicates an absence of the criterial attributes required for categorizing someone as being thin. 'If' is no more problematic than the terms already brought into consideration. 'If' in "If something is a rose, then it is a plant" indicates that the set of attributes required for labeling anything a rose always appears conjoined with the criterial set of features needed for correctly applying the word 'plant'. Thus, the meaning of 'or', 'not', and 'if' is no more mysterious than that of 'table'. It is simply that logical meaning refers to the co-occurrence patterning of sense experience, especially insofar as that patterning reflects the 'objective' order of things as a system of constant and systematic interconnections of possible experiences valid for any given perceiver. And it is, of course, the rule governed functional unification of perception and memory which underlies the possibility of recognizing the objective order of experience. In regard to logic one must remember that man's innate psycho-physical apparatus developed as it did due to the necessity of providing the basis for adequately orienting man in the physical world which holds the key to man's survival; and the *sine qua non* of man's orientation in the physical world is the ability to recognize the objective order of things given in sense experience as opposed to the subjective order of our personal experience of that reality. Needless to say, since we cannot transcend our innate psycho-physical apparatus, the nature of logical meaning in regard to any conceivable worlds will necessarily be the same as it is in regard to the world in which we exist.

The fundamental validity of our assumptions concerning the unity of thought and perception and the consequent role of logic, can be brought into perspective by an examination of the nature of mathematical meaning. And since we have maintained that logical meaning has as solid a foundation in sensory experience as does lexical meaning, it will be illuminating to begin with Bertrand Russell's observation on what he considered to be the results of nineteenth and twentieth century work on the logical foundations of mathematics. According to Russell, one of the consequences of this work

> is to dethrone mathematics from the lofty place that it has occupied since Pythagoras and Plato, and to destroy the presumption against empiricism which has been derived from it. Mathematical knowledge, it is true, is not obtained by induction from experience . . . . But it is also not a priori knowledge about the world. It is, in fact, merely verbal knowledge. "3" means "2+1", and "4" means "3+1". Hence it follows (though the proof is long) that "4" means the same as "2" + "2". Thus mathematical knowledge ceases to be mysterious. It is all of the same nature as the "great truth" that there are three feet in a yard. (1945:831-32)

Mathematics, then, according to Russell (1945:155) is analytic, and thus "pure mathematics consists of tautologies, analogous to 'men are men', but usually more complicated". The question then arises as to just what constitutes the basis of analytic knowledge in general. One of the best analyses of the problem of analytic knowledge has been provided by C.I. Lewis. Lewis (1946:146) very ably pointed out that language, and symbolic systems in general, do not exist "for the sake of supporting a language habit and exemplifying concretely our chosen forms of syntax" but rather for the sake of guiding our identifications of sensory experience (cf. 1946: 141). This means that the basis of analytic meaning necessarily lies

> in the relations of testable and sense-recognizable characters which are our criteria of classification and of the application of verbal expressions. One such essential property signified may include or entail another; as the criterion of squareness includes the criterion of rectangularity, and the applicability of 'cat' entails the applicability of 'mammal'; and one such may exclude another as incompatible; as satisfaction of the test of squareness excludes satisfaction of the test of circularity, and as the applicability of 'cat' excludes the applicability of 'fish'. (Lewis 1946:156)

This ties in beautifully with what we established in regard to the nature of logic in general. Thus, analytic meaning has a solid foundation in sensory experience, and is completely nonproblematic within the context of the theory we are advancing. Moreover, the relationship of mathematics to logic is no longer mysterious once one understands the nature of logic. For both mathematics and logic deal with the structure of sensory experience. Numbers represent rule governed units of perceptual discrimination, with similarity (equality is a special case of similarity) ultimately constituting the basis of these rules, just as it does all other rules. Thus, it is very revealing that research has shown primitive number systems often have their foundation in the set of fingers and toes each of us has, with these sets of physical digits providing the basic units of comparison (cf. Cassirer 1964). Such systems do not reflect a different mentality but rather the perceptual foundation of mathematics. Once we realize that the number

ten represents nothing more than a prescribed set of possible strokes of perceptual discrimination, then the possibility of deriving a number system from the number of fingers on both hands becomes apparent. Ten, for example, simply represents the set of strokes of perceptual discrimination which is the same (1=1) as the set of strokes of discrimination required for one to focus separately on each of the fingers of both of his hands. In general we would suggest that numerical concepts reflect the active role of the perceiver in the process of perception. And as in the case of meaning in general, it is clear that we will not get far until we analyze the sensory conditions which are criterial for the application of numerical concepts to experience. Certainly mathematical knowledge can, as Russell argued, be regarded as analytic in nature; but, as we have seen, all analytic meaning is ultimately based on the relations of the sensory criteria which constitute the basis of the applicability of concepts to experience. This applies to mathematics no less than it does to logic. Anyone familiar with Kant's views on the foundations of mathematics will immediately recognize that what we are suggesting in regard to the nature of mathematics represents a reinterpretation of the Kantian standpoint within the context of the theory of mind and meaning being advanced in this study. For Kant fully realized that mathematical meaning is ultimately founded in regulated perceptual activity (cf. Körner 1960).

That there is an intimate connection between logic and grammar has been recognized since antiquity. Consequently, if our theory concerning the nature of logic and the unity of perception and thought is correct, then our theory should provide an adequate foundation for dealing with the problem of grammatical meaning in general. This, we believe, is the case. Consider, for example, the copious evidence which led Leibniz to assert in his New Essays Concerning Human Understanding (3.1.5) that prepositons signifying logical relationships originally signified sensory relationships, with most originally signifying relationships relating to the spatio-temporal ordering of sensory experience. Consider also that the same pattern of derivation which Leibniz observed in regard to the use of prepositions, also demonstrably holds true for morphological case markers. In fact, the evidence is so compelling that a number of first-rate scholars, including Brugmann (1911:473), Delbrück (1901:133), Whitney (1882:91), and Cassirer (1964:161-62) all maintained that all morphological case markers originally denoted sensory relationships, with some even arguing that all case markers originally denoted spatial relationships. Although the ubiquity of this pattern of derivation strongly suggests that it reflects a fundamental characteristic of the relationship of language to the mind and experienced reality, traditional attempts at explaining this pattern have been conspicuously without success. This lack of success should not be too surprising. For implicit in the traditional analyses of this pattern of derivation is the assumption that there is a fundamental difference between the allegedly purely conceptual grammatical relationships and all sensory relationships. Such an assumption is, of course, ill founded, and provides a totally inadequate framework for explaining how terms originally denoting purely sensory relationships came to denote purely grammatical relationships. The answer is simple: there is no dichotomy of sensory

relationships and grammatical relationships. Purely grammatical meaning is itself totally founded on sensory criteria. Thus, the reason that terms originally obviously denoting sensory relationships came to denote what we label grammatical relationships, is that there was at least some partial overlap between the set of sensory criteria originally signified by the term and the set of sensory criteria which governed its use as a term denoting a grammatical relationship. In the course of time the two uses of the term could drift far apart enough so that the same degree of overlap no longer existed. This would mean, of course, that future generations would have no basis for understanding this derivational pattern. Thus, the pattern of derivation noticed by Leibniz in regard to prepositions, and by such scholars as Brugmann, Delbrück, Whitney, and Cassirer in regard to the derivation of morphological case markers, makes a great deal of sense within the context of the theory we are advancing.

If our assumptions concerning the unity of perception and thought as reflected in the nature of logical and grammatical meaning are correct, then it should be possible to shed further light on the underlying basis of the interdependence of syntax and semantics in ordinary language. This, we believe, is the case. For it seems that both paradigmatic and syntagmatic patterning are ultimately based on the patterning of sensory experience. The co-occurrence patterns of nouns in regard to verbs and adjectives provide a convient point of reference in regard to this issue. For, as Walter Porzig and many others have observed, given nouns can co-occur only with certain verbs and adjectives. The underlying basis of this patterning becomes clear when one examines the problem of reference. For disembodied actions and qualities are not to be found in immediate experience. Instead, actions and qualities are found only as differentiating aspects of 'things'. Thus, for example, one will never encounter a 'dancing' as an entity with a segregated givenness in sensory experience, although one might well see a bear dancing. Nor would one ever encounter a 'tall', but rather tall things or people. Consequently, since the denotata of adjectives and verbs occur only as differentiating aspects of entities with a segregated givenness, and since entities with a segregated givenness are generally denoted by nouns, then it is clear that the restrictions in regard to which nouns can co-occur with which adjectives and which verbs, are ultimately based on whether the segregated entities denoted by the nouns can manifest the 'actions' or attributes denoted by the given verbs and adjectives. Although it might seem that only spatial entities can have a segregated givenness, this is not the case. Figure-ground differentiation is perhaps the most ubiquitous mode of perceptual organization, for some form of figure-ground differentiation is possible in all sensory modalities. As Walter Ehrenstein 1965 astutely pointed out, by focusing our attention on a given aspect of sensory experience, we can achieve a much greater degree of awareness of that particular aspect, with this aspect becoming the focal point of our consciousness, and thus being set off from the rest of the given perceptual field which then constitutes the ground. Consequently, we can focus on any aspect of our sensory consciousness in such a way as to experience it as a segregated entity. This applies, of course, to the attributes and actions,

aspects which normally do not have a segregated givenness in experience. Thus, phenomena which would normally belong to either the verbal or adjectival domain of reference can be focused on in such a way as to become the focal point of our awareness and thus achieve a segregated givenness in terms of figure-ground differentiation and consequently belong to the nominal domain of reference. From the foregoing, it is obvious that we believe Roger Brown (1957:3) was correct in suggesting that the part-of-speech membership of a word operates as a perceptual filter guiding our attention towards the relevant aspects of the phenomena being classified. Indeed, within this framework one can easily explain the meaning of the part-of-speech membership of words. For once one realizes the importance of figure-ground differentiation in this context, nouns constitute no problem--nouns characteristically denote phenomena with a segregated givenness in sensory experience. And verbs can be considered as characteristically denoting those temporal aspects of segregated phenomena which manifest qualitative or spatial change (or absence of change) in time; while adjectives characteristically denote attributes without regard to whether or not they are manifesting some sort of change in the process of time. Shifts in form class, as we have already indicated in our discussion of nouns, can easily be explained in terms of shifts in the sensory criteria required to correctly use a term. Needless to say, the classification of form class meaning given here is meant to be merely illustrative and not in any way exhaustive.

In general, we believe that in providing the foundation for correlating the structure of perception with the structure of lexical and grammatical meaning, the proposed theory furnishes a universally valid framework for comparing and contrasting the way in which different languages segment reality, since perception is the same for all men regardless of language to the extent that what is experienced by the users of any one language is <u>potentially</u> experienceable by the users of any other language. Concepts are ultimately rules of sensory identification, and thus contrastive analysis can be based on the systematic comparison of the differences and similarities in the sensory conditions criterial for the use of any given lexical or grammatical concepts in any languages which are to be compared. Interestingly enough, Ernst Leisi 1967 has already shown the validity of such an approach in his contrastive analysis of certain segments of the English and German lexicons. The theory we are advancing shows that the sort of linguistic analysis Leisi has already undertaken within a relatively limited domain, is in fact possible in all domains of meaning, both lexical and grammatical.

Since, as we have shown, one can make a good case for explaining the unity of thought and perception within an empiricist framework, the question arises as to why this line of investigation has not received more serious attention in the recent past. Perhaps one of the most important reasons lies in the confusion concerning the epistemological implications of modern physics. Therefore we will briefly relate our theory to the most important considerations in this area. Within the theory we are advancing the possible modes of resemblance of the content <u>and structures</u> of sensory experience constitute the set of possible distinctive features. However, as

we have already indicated, this does not mean that conceptual constructions qua constructions need be copies of what is given in immediate perceptual experience, but only that the ultimate constituents of these constructions are derived from immediate sensory experience. Thus we would agree with Köhler's observation that

> even the most foreign symbolic language in which at present the theorist makes his statements on quantum physics remains physically meaningful only to the extent to which this language points ultimately back to some phenomenal sources and to possible verification in perceptual situations. If some property . . . should prove unintelligible in any phenomenal terms, it would necessarily remain inaccessible forever. (1938:172)

We have dealt with this problem complex in regard to the nature of space and time elsewhere (cf. Slagle 1974b). In this context, we would note that most of the difficulties in regard to the problem of time stem from the failure to adequately distinguish between psychological time, that is, time as it is given in immediate perceptual experience, on the one hand, and the time of the physicist, on the other. It is, as William James (1950:vol.1,631) asserted, the time of immediate perceptual experience which ultimately constitutes the basis of all conceived times. The time of immediate perceptual experience is not, however, to be confused with the time of Newtonian physics (cf. Bertalanffy 1955:259). The failure to make this particular distinction has led to much of the unnecessary confusion which has continued to surround the epistemological implications of modern physics and obscure the validity of the standpoint represented in this study. But as we have shown in another study (1974b), such difficulties are easily resolved within the framework of the suggested theory.

In this study we have attempted to demonstrate that there is a viable empiricist alternative to Chomskyan rationalism. We have attempted to show that there is an underlying unity of perception and thought, a unity which is totally explicable in terms of the structure and dynamics of sensory organization. The experimental findings of Gestalt psychology have, of course, played a crucial role in our considerations. Here we would like to note that the principles of spontaneous unification in sensory fields discovered by the Gestalt psychologists constitute facts, facts which cannot be explained away simply because no one has come up with an adequate explanation of their neurophysiological basis. Indeed, any adequate neurophysiological theory will have to be able to explain such field effects; for without positing such field effects it is impossible to explain how we can apply concepts to new experiences which differ considerably from any previously experienced instances of the given categories in question. Thus, it is far more than just a coincidence that similarity and contiguity, long thought to underlie the dynamics of categorization and memory, have been shown to be factors influencing spontaneous unification in sensory fields.

In sum, there are solid reasons for believing that one can explain the underlying unity of thought and perception within an empiricist framework. And within such a framework lies the basis for

a far richer analysis of language and mind than has previously appeared possible.

NOTES

[1]Very useful general discussions on the role of similarity as a principle of spontaneous unification in sensory fields are found in Wertheimer 1923 and Köhler 1947. As both Köhler and Wertheimer emphasized, the principles underlying spontaneous unification are not limited to any given sense modality.

[2]For information on the experiments which prove that similarity, functioning as a principle of spontaneous unification, is operative in the structuring of stroboscopic movement, see especially Schiller 1933. Krech (1969:223) offers excellent illustrations of the relevant principles of spontaneous unification involved in structuring stroboscopic movement. Consult also Wertheimer 1923, Koffka 1935, and Köhler 1940.

[3]Good examples of Wertheimer dots are found in Wertheimer 1923 and Köhler (1940:61-2). Both sets of examples should be consulted. Unfortunately, the adaptation of Wertheimer dots in Krech 1969 is poor.

[4]Interestingly enough, good illustrations of the influence of context on perceived size are found in Pribram (1971:134).

[5]On the striking spontaneous reorganization often experienced, see Thurstone (1944:20).

[6]The functional interaction of memory and perception, which is exemplified in Gestalt completion phenomena, also often underlies the way in which we mishear or misread. Consider, for example, the difficulties one has in proofreading one's own manuscripts.

[7]Walter Ehrenstein (1954:317-18,1956:72-73,1965:207) fully recognized the validity of this viewpoint. He did not, however, adequately deal with the all important issue of the nature of the interaction of memory and perception, which is the sine qua non of an adequate theory of mind.

[8]See Gazzaniga (1970:105), and Levy, Trevarthen, Sperry 1972. Anyone who believes that these studies do not show that one can recognize Gestalten using only the language dominant hemisphere, does not know what a Gestalt is.

[9]Similarity plays a crucial role in Mill's theory. See Mill's notes to his father's Analysis of the Phenomena of the Human Mind, vol. 1, pp. 111-14, 260-63. This work gives the reader the best insight into Mill's mature position.

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ON "ADEQUACY" OF DESCRIPTION AND EXPLANATION IN THEORIES OF LANGUAGE ACQUISITION

Recent work in linguistics has been severely hampered by the misunderstanding and resultant misinterpretation of Chomsky's remarks on the notions of "descriptive adequacy" and "explanatory adequacy," as these notions apply to the search for a systematic theory of language acquisition. Chomsky seems to insist that linguistic theory can develop productively and proceed to the clear formulation of "deeper questions" only if scholars will consider first the abstract problem of constructing an "acquisition model" for language--that is, a theory of language learning or grammar construction. "Adequacy" of description and explanation is crucial in such a theory, it is suggested, because the structure of such a primary model must be both perfectly efficient and all-inclusive.

As a point of departure, let us cite the most extensive statement of this position, along with an important subsequent modification:

> Clearly, a child who has learned a language has developed an internal representation of a system of rules that determine how sentences are to be formed, used and understood. . . . we can say that the child has developed and internally represented a generative grammar, in the sense described. He has done this on the basis of observation of what we may call *primary linguistic data*. This must include examples of linguistic performance that are taken to be well-formed sentences, and may include also examples designated as non-sentences, and no doubt much other information of the sort that is required for language learning, whatever this may be. . . .[1]

More recently,[2] Chomsky has amended this emphasis on observed primary linguistic data by including a posited set of "linguistic universals" of which the child has a tacit knowledge:

> A theory of linguistic structure that aims for explanatory adequacy incorporates an account of linguistic universals, and it attributes tacit knowledge of these universals to the child. It proposes, then, that the child approaches the data with the presumption that they are drawn from a language of a certain antecedently well-defined type, his problem being to determine which of the (humanly) possible languages is that of the community in which he is placed. Language learning would be impossible unless this were the case. The important question is: what are the initial assumptions concerning the nature of language that the child brings to language learning, and how detailed and specific is the innate schema (the general definition of "grammar") that gradually becomes more explicit and differentiated as the child learns the language?[3]

Here the intention is to outline a model grammar which may provide an account of this tacit knowledge, or the "specific innate abilities"[4] that make language acquisition possible. Adequately described, such a grammar will demonstrate the notion of "descriptive adequacy," as Chomsky has outlined it, and furnish at the same time an adequate explanation of "explanatory adequacy."

To simplify, let us consider a possible sentence (S_1) as the absolutely primary linguistic datum provided as input to an embryonic optimal finite perceptual device, of the type discussed in Chomsky (1959a). We shall refer to this perceptual device henceforth as "Wunderkind" (W_1).

Suppose $S_1 \longrightarrow S + CS$

where CS $\longrightarrow$ Contextual Supplement (non-verbal, whatever this may be); that is, audible gestures, such as snapping the fingers, patting heads, stamping the foot, or, as in this case, administering a "spank."

Suppose further that S $\longrightarrow$ "Say! Ten pounds, I bet!"

Let W_1 represent the approach of Wunderkind to this data.

As Chomsky has rightly observed, his problem is to determine from which of the (humanly) possible languages S_1 has been generated. This is not a simple problem, to be sure. It could be convincingly argued that S_1, if (1) implies unambiguous priority, is an utterance of low probability on two grounds: 1) It is well known that a perceptual device of the type W_1 receives sensory input while still technically a subset of M (Mother), but that such input (for example, S) is subject to unique transformations of the sort IM)) (intervening membrane) and WT ◎ (womb tone). 2) Further, very few examples of W_1 attain a verifiable magnitude of ten pounds until *after* these transformations (IM)) and WT ◎) are completed. Hence, if S is indeed unambiguously prior, it is likely to be misunderstood, due to the so-called "muffling" transformations, and is therefore *not* unambiguous. If, on the other hand (henceforth, "on the one hand" $\longrightarrow$ ⟩— and, "on the other hand" $\longrightarrow$ —⟨) S_1 is clearly understood and not therefore ambiguous, it is probably not prior to utterances of the type "It's gonna be a whopper, Phyllis," or "Get 'im a contract with the Packers now," or "I hope to Christ it isn't a girl."

But assuming the most likely case, where S undergoes IM)) and WT ◎, we may find W_1 unable to identify his linguistic community. He may apprehend S as

C'est---Tiens! Pa' une d' ces bêtes!

or even

zehn Panzer im Bett!

There is the additional possibility of his correctly identifying the linguistic family, but not through a highly specific detailed innate schema, but by a quite arbitrary reversal of the general proposition, applied in this case to IM)), that Interference Blurs Meaning (IBM), or, as in this case, a "B" intervenes between the (I) and (M),[5] as a special case of the intervening membrane of meaning transformation, applied to itself. To cite a hypothetical but feasible application of this general proposition, W_1 may apprehend S as

SATAN PAWNS EYE BET

that is, the headline story in the other world, where millions queue up to buy the five-star final and follow the fascinating chess tourney between the Red Devils, sponsored by cunning linguists, and the mystery team--dubbed Sin Tax by the press--and wager on their favorites, pseudo-Jansenists that they are! (Not to know how diabolically clever it was of Pascal to offer the gullible sinners one more chance, gambling himself with the offer, is to join the ranks of doomed addicts of *le sport*, those who merely *play*.) Consequently, W_1 may infer wrongly (humanly) that he has a chance to fool around a little before the next datum he must approach, because he has, indeed, the right language, but for the wrong reasons. Language learning would be impossible unless this were not the case.

It may be asked, what tacit universals, or initial assumptions about language, justify W_1's interpreting "Satan pawns" as an example of infantile journalese, analogous to "Da-da car" or "Wundee trike," or, to maintain moral balance, "Jesus freaks"? Why doesn't the little bugger imagine that Old Nick made an eye/tooth paramutual with Mr. Big, and hocked his potential winnings; that is, sold redeemable futures in Manichee Unlimited to every opthalmologist in town? Has he been curled up in there for months reading an invisible *Times*?

The answer to this question, as to several others we have touched on indirectly, is to be found in an analysis of the model grammar designed to meet our original criteria of ⟩——"descriptive adequacy," and ——⟨"explanatory adequacy."

Hence:

$\infty \longrightarrow$ if + mind

$$\text{mind} \longrightarrow \left\{\begin{array}{l}(\text{you})^{6} + \text{mind}^{7} \\ \text{mind} + (\text{-ing}) \\ \text{Alfred} + \text{Cleo}\end{array}\right\} + \text{mind}$$

SD: you + mind
SC: you + do + not + mind
SD: mind + -ing
SC: not + mind + -ing
DSC:* not + do $\longleftrightarrow^{8}$ do + nut

Thus:[9]

The grammar can degenerate, from zero to infinity, and include infinite degrees of the mindless, if

mind	mind mind mind mind mind mind mind mind
you mind	mind you mind
you mind you mind	mind mind you mind
you mind minding	you do not mind minding
you mind mind minding	you do not mind mind minding
you mind mind minding mind	you do not mind mind minding mind
you mind you mind mind minding	you do not mind not minding you mind
you mind you mind mind minding	you not mind I not mind

*Awarded to all who have come this far.

NOTES

1 Noam Chomsky. (1965). *Aspects of the Theory of Syntax*, pp. 24-25.

2 On the next page, in fact. *Ibid*, pp. 27-28.

3 10 ∃s, *ibid.*, 27-28.

4 As throughout *Cartesian Linguistics* and *Aspects*.

5 Here also is an example of Irony of Inversion, since, clearly, "Be" is more general than "I AM."

6 The embracing arms mean, you can take me if you want (to), but if you do you have to take all of me. You have to take everyone else; they're armless.

7 The larger, more byzantine embracing arms mean, you'd better take one of us or you'll be laid waste from here on.

8 Siamese Spike. The old oriental sailor always orders a donut.

9 Contraction for "the us"; that is, the small group of us who are still here.

MADISON AVENUE ADVERTISING: A SCENARIO

Adam Makkai
University of Illinois at Chicago Circle

1. The reader may justifiably wonder what Madison Avenue Advertising has to do with a discipline such as linguistics. Since I do not intend to give away my punchline, I must ask the reader's indulgence in bearing with me until the connection becomes self-evident. I really think it is.

2. Let us look at some case histories.

2.1.The case of *Scope* or how to frighten people into buying my brand of mouthwash. I imagine most people reading this paper have seen the advertisement (and its allos) on the television screen. A typical one goes like this: A worried employee, whose boss has 'bad breath' is trying out various inoffensive sentences practicing, as it were, for the moment when he (or she) works up enough courage to present the boss with a bottle of *Scope*. The faces of the actors and actresses vary; but the scene is usually an office, a school, or some such setting. Invariably, just before the person actually manages to give the boss the *Scope,* the boss walks in, -*Deus ex machina*- and informs the bewildered employee that he just discovered the pleasures of using *Scope. Never before did my breath feel so fresh!* -he intones triumphantly, and the sufferer of the boss' 'bad breath' sighs with mighty relief.

2.2. Witness the case of the crowded Volkswagen and the fourth member of a car pool who is not enlightened enough to use *Dial,* a popular brand of soap. The small VW arrives in front of the fourth person's home. He is portrayed as a slovenly person; illkempt, hurrying, late. The three men in the VW have to wait for him, but as they still wait, their faces are reasonably calm and serene. Now the fourth man arrives. The faces inside the VW become disgusted; the fourth man literally stinks. They turn their heads away from him, as the VW pulls away with a jerk. The announcer says: *Aren't you glad you use Dial? Don't you wish everybody did?*

2.3. We are witnessing the promotion of a popular brand of small cigar, the sophisticated *Winchester*. An ordinary looking fellow is busy cleaning up around the cage of some wild animals; this is a circus. A most dazzlingly beautiful woman, officially 'belonging' to the ordinary looking person, lingers about. At this moment Mr. Handsome arrives. He has hair, but not too much, no, he is not a hippie. He wears Western style boots, expensive looking thick, custom tailored shirt and a fancy, Hollywood-style cowboy hat. He looks cunning, successful, spirited --- the very opposite of the other man. He watches the man and the woman, then slowly approaches the latter, offering her a *Winchester,* as he, too, lights one up. She immediately accepts with a glitter in her eye ---infatuation, if not love at first sight. They hold hands and amble off towards the barn, while the handyman remains there alone. That is- not quite, for Mr. Handsome having acquired the lady, leaves him his *Winchesters*. Mr. Drab looks wistfully after Mr. Handsome and then, as if struck by sudden insight, picks up the

package of *Winchesters* as he keeps shifting his gaze between the inherited cigars and the disappearing couple.

2.4. We are back in the days when cigarette advertising was still legal on television, only a few years. We have all seen these advertisements. I will only concentrate on two here, *Silva Thins,* and their female counterpart, the *Virginia Slims*.

2.4.1. This is the *Silva Thin* advertisement. Total running time: one minute. We are in the compartment of a train, somewhere in Europe. Cypresses and castles are seen through the window. Mr. Handsome, wearing a fashionable white turtle neck sweater, stream-lined sun glasses, and an elegant sports jacket, looks out the window. Opposite him sits a strikingly beautiful woman. They don't seem to be familiar with one another. Between the two of them, on a small table under the window, lies a pack of *Silva Thin* cigarettes. The woman surreptitiously reaches out for them, since he has deliberately not offered her any, or made any other overtures for conversation. Does he say *sorry Ma'am,* and hasten to offer her one, with his lighter or match lit? Definitely not. That would be the wrong Mr. Handsome. This Handsome fiercely grabs the woman's hand and rescues HIS OWN favorite *Silva Thins*.He takes one, lights it, smugly turns away from the confused and frustrated woman. He looks out the window triumphantly, she sinks into herself, defeated.

2.4.1.1. An allo of the *Silva Thin* plot takes us to Venice, every lover's dream city, the city of canals and gondolas. Handsome and Beautiful arrive at the pier where there is a gondola waiting. The viewer expects that the two, being American lovers on a European junket, would leap into the gondola together, holding hands. But no. Handsome holds out a few dollars which the gondolier avidly grasps; then Handsome ushers Beautiful into the gondola, while the gondolier quickly paddles away with her on board. Consfused, she stares after Handome in bewilderment who, triumphantly and glad at having gotten rid of HER, lights a *Silva Thin*. (The camera, of course, zooms in on the product so the viewer cannot mistake it for any other.)

2.4.2. This is the female counterpart of the *Silva Thin* plot, the *Virginia Slim* cigarette, designed to attract the attention of feminist women. A typical version used to go like this (and is now repeated on the pages of magazines, since it is forbidden by Congress to be shown on television): A middle class housewife, dressed in buttoned shoes, corset, and laces, smokes a cigarette in great anxiety ---while sitting on the toilet in the bathroom. She makes rapid motions with her hand in order to drive the smoke away, lest her (presumably disapproving and oppressive) husband notice what she is up to. She hears footsteps; her husband must be on his way up. She throws away the cigarette, half smoked. Cut to 1970. We hear the *Virginia Slim* music sung to the words of *you've come a long way baby, you've come a long, long way*. A *Cosmopolitan* covergirl fashion goddess parades in front of the viewer on the screen, dressed in miniskirt, boots, covered by expensive fur coat. She is leaving an office building of some sort where, presumably, she works. She lights HER OWN *Virginia Slims,* and once again the camerman makes sure that the name of the product registers with the viewer visually.

2.5. But there are some counter-advertisements as well. Consider the following two advertisements AGAINST SMOKING sponsored by the American Cancer Society:

2.5.1. A little boy, no older than three, sits next to his father who is driving his car. The little tot has his own toy steering wheel, and executes steering motions, as he watches his father drive. We hear the voice of the announcer: *Like father, like son.*

severe and rather sinister: They are designed to brainwash the viewer into buying the product by using SEDUCTION (a positive stroke) and INTIMIDATION (a negative stroke), to use the familiar terms associated with transactional psychoanalysis Berne(1966, Harris 1967).

3.1. I will call this type of Madison Avenue advertising a SPELL or an INCANTATION. The reason: It is designed to interfere with the consciousness of the viewers without their consent. But that is precisely the anthropological definition of classical, Salem-style MALEFIC WITCHCRAFT! Hansen (1969) distinguishes between malefic witchcraft and benefic witchcraft exactly on this basis: Has the PATIENT consented to the spell-caster's altering of his consciousness, or hasen't he? If I teach my six-year old daughter how to draw the big letter L as in *LOVE* I, too, have 'interfered with the consciousness of another'. But the purpose was quite different. Either she asked me to teach her, or it was my best, unselfish judgment that giving her the relevant information about the capital letter, the grapheme *g/L/* was, under the circumstances, in her best interest. In short, I have TAUGHT her something.

3.2. I will call the American Cancer Society's ad with the little boy trying always to imitate his father an EXORCISM. It runs the four-part drama (psychologically) backwards; what has been shown first as a strong association is DISSOCIATED at the end when the announcer's voice picks up the interrogative intonation leaving the viewer wondering whether his little son should indeed imitate him in ALL matters.

4. And now to our tie-up with modern day linguistics. I wish to argue here that the strategy of many transformational-generative grammarians has been essentially identical to the cumulative plot-structure of these advertisements discussed above. Individuals and their styles vary, of course, but almost all of the themes are reducible to a similar four-part scenario with a protagonist and his goals; with a (frequently fictitious) antagonist with his (frequently fictitious) goals and purposes; the introduction of the new theory as the merchandise, followed by the sometimes subtle, and sometimes embarrassingly obvious catharsis of the respective payoffs. This is how it works:

4.1 The protagonist declares that he has something REALLY NEW and REVOLUTIONARY to say. His situation and circumstances are of the brave underdog who just fought his way out from under oppression and obscurity, scorned by the Old Establishment. His theatrical garb includes the good looks of (a) mentalism, (b) theory-orientedness, (c) courage, (d) insight, (e) progress, etc. The antagonist, on the other hand, is made to appear very unattractive, for he is (a) mechanistic, (b) a mere data gatherer, (c) hangs on slavishly to the Old Establishment, (d) lacks insight i.e., descriptive and explanatory adequacy; all he has is observational adequacy, if he has that; (e) in short, he is a reactionary; probably also stupid. This may be amplified by involvement on the current political scene: The protagonist is a brave war resister, a daring left-wing liberal; with the antagonist being a dull, unimaginative member of the 'silent majority'. The *piece de résistance* introduced has, of course, great intellectual sex-appeal, just as does *Dial* soap, in whose absence we turn our noses up and away. It is the discovery that English is not a finite state device; that transformations and ordered rewrite rules are necessary to relate one sentence to another; that underneath the 'surface structure' there is a 'deep structure', etc.
The sales strategy establishes the following associations:

(1) Sentence syntax is the central axis of language.
(2) If ambiguity is seen in the meaning of a sentence, there must be two non-ambiguous SENTENCEOID deep structures (or 'semantic representations' in the generative semanticist framework) from which the respective meanings of the ambiguous surface sentence can be derived.

The man stops to fix a flat tire, and the little boy happily hammers along with him. The announcer repeats: *Like father, like son.* They arrive at a picnic site near the highway. The father opens a sandwich bag and and bites into his food. So does the little boy. Again the announcer repeats (somewhat faster and with a raised pitch): *Like father, like son.* After finishing the sandwich, the father reaches for a cigarette and lights it. The little boy looks at his father and, as before, is about to imitate him. His arm reaches out for the cigarettes, when the picture freezes the motion in mid-air. We hear the announces ASK the question with a much changed, heavily disapproving intonation: *Like father, like son?*

2.5.2. The following ad did not run long; in fact very few people saw it. It was shown on a television special of cancelled American Cancer Society advertisements. The popular actor who used to play Mr. Burger, Perry Mason's opponent in the popular television series *Perry Mason,* who, in his capacity of District Attorney for the Prosecution invariably loses his cases to the witty Perry, comes on as his real self, pained and haggard looking and says: *You know me. I am a loser. Do you know why? Not because of Perry Mason, that was just play. This time I am a real loser. I am dying of lung cancer. Please do yourself a favor. Stop smoking.* (And as we all know, he did die.) I need not elaborate on the devastating effect this ad would have had on the cigarette industry, had it been run as often as, say, the *Silva Thin,* or the *Virginia Slim* advertisement was.

3. There were hundreds of advertisements like this, and there still are. The American public must live with this massive onslought of false-association-creating, day by day. There is no need to evaluate them all. Look at any billboard asking you if you have ever *TOUCHED BLACK VELVET*, where *Black Velvet* is, of course, the name of a whiskey (also known as The Smooth Canadian) and, not quite coincidentally but very deliberately, there is a Hollywood goddess reclining on her sofa wearing just that, black velvet, holding on to a bottle of The Smooth Canadian, on the bottle-neck of which there is a small patch of black velvet 'rhyming' visually with what the woman wears. The point of this paper is that this type of Madison Avenue advertising, built on what is euphemistically called 'motivational research', is an old POLITICAL PLOT every single instance of which is reducible to a small four-act drama:

ACT I: We see the *PROTAGONIST,* and his situation and circumstances.
ACT II: We see the *ANTAGONIST,* learn of his purpose and goals.
ACT III: *We are introduced to the* PIÈCE DE RÉSISTANCE, *that is, the* MERCHANDISE.
ACT IV: Both the protagonist and the antagonist get their respective PAYOFF, for the benefit of the audience, who are urged to choose between the portrayed positions.

In the *Silva Thin* ad, for instance, the protagonist is Handsome, and the antagonist is Beautiful. The merchandise is, of course, the cigarette. The protagonist and the antagonist clach over IT. The respective payoffs: He projects the image of the free, strong, masculine man, the very opposite of the hen-pecked husband, and she is cut down to size; is put in her place. The same goes for the feminist cigarette, only in the reverse. In the case of the *Winchester* cigars, the protagonist is Handsome, the brave challenger and the winner, who HAS WHAT IT TAKES TO MAKE IT in our society. The antagonist (and therefore the loser) is the handyman, and the merchandise is the cigars, whose supposed acquisition can CHANGE your image from that of loser to that of winner. The payoff is the Hollywood goddess with whom the winner ambles off toward the barn. In the case of the *Dial* soap ad the three men in the VW are the protagonists (they use the right soap) and the fourth man is the antagonist. The merchandise is the inevitable soap whose absence in the aura of the loser is keenly registered on the faces of the protagonists. He who uses *Dial* is liked by his fellow men, he is OK (all positive adjectives fit); he who does not, is NOT OK, he literally stinks. The implications are

(3) The surface sentence relates to 'performance' as deep structure relates to 'competence'. This is important because sentences that are not acceptable as grammatical AS THEY ARE, might be judged to be grammatical in their 'deep structure' form. (I.e. what you SEE and HEAR doesn't count, it is what the IDEOLOGY TELLS YOU that counts.)
(4) Surface structure is to mechanism and mechanical data gathering, as deep structure is to mentalist theory-orientedness. Surface-oriented linguists are stupid, do not deserve to be published, should not be hired or promoted; theory and deep structure-oriented transformationalists, on the other hand, deserve full professorships at the ages of 27 or 28; immediate publication of student term papers in prestigeous journals; promotion and tenure within a very short time after graduation.

5. The negative strokes are so powerful and the positive ones so seductive; the sales-pitch is so cleverly intellectual and so self-servingly critical of the opposition, that almost nobody can resist it. The SCRIPT OF THE MOVEMENT ensnares and exploits you. If you do not jump on the bandwagon, you are stupid; if you do, you become automatically an intellectual hero.

5.1. There have been attempts at exorcism against this script. Consider the famous flier sent out to scholars by Professor W. Freeman Twaddell:

SURFACE STRUCTURE IS TO DEEP STRUCTURE

AS PERFORMANCE IS TO COMPETENCE

AND AS BEHAVIOR IS TO MIND

AND AS OBSERVATION IS TO SPECULATION

AND AS DEMONSTRATION IS TO IPSE DIXIT

AND AS PUBLIC VERIFIABILITY IS TO PRIVATE INTUITION

AND AS SCIENCE IS TO MYSTICISM.

WFT

The success or failure of this kind of exorcism depends on whether or not the reader believes that 'mysticism' is a BAD THING and whether it indeed compares to the item to be exorcized, that is, deep structure. As a stratificationalist, I can go along with Twaddell's first six lines, but the seventh, his punchline, I must reject, because for me -since the Heisenbergian Indeterminacy Principle put the observer back into the picture of the act of observation reiterating, as it were, the Kantian 'Ding an sich' in the language of modern subatomic physics (cf. Makkai 1972 and Lukacs 1968) - science and mysticism are not NECESSARILY contradictory terms. This is particularly true in the wake of space travel, modern astronomy, and parapsychology. (Cf. Wescott 1969). In his collected Harvard Lectures, William James (cf. Makkai 1972) successfully re-

solves such 'opposites' as science and mysticism. I must further disagree with Twaddell because I have evidence to the fact that the meaning of a sentence does not necessarily follow from the structural parsing of its surface syntax; though I do agree with Twaddell that the meanings of ambiguous sentences NEED not be derived from a 'deep structure' via transformations. I regard, therefore, Twaddell's flier (as reproduced above) as a semi-exorcism, or rather a counter-incantation, issued by the Old Guard against the Young Turks.

In Figures 1,2,3 and 4, overleaf, I would like to present a much more effective exorcism of the deep-structure versus surface structure issue in terms of a stratificational-cognitive analysis.

6. Figure 1 shows the 'surface structure' of the sentence *visiting relatives can be a nuisance*. Stratificational Grammar recognizes the fact that NEUTRALIZATION can occur between two different SEMEMIC TRACES on the LEXEMIC STRATUM. Hence the immediate constituency diagram of Figure 2 can either be a REALIZATION of SEMEMIC TRACE $(ST)_1$ or of ST_2.

Figure 2, ST_1, shows the 'present active participle' interpretation of the sentence, where *visiting* modifies *relatives* and *relatives* is the subject of the sentence, with *can* realizing the sememe ^{s}/plural/. Figure 3, ST_2 shows the 'gerundival interpretation' of the same sentence, with *can* realizing the sememe ^{s}/singular/ and *relatives* being the direct object of *visiting*, nominal in the singular with the function of a transitive verb.

Notice that in order to understand that the same sentence can have both meanings, we do not have to resort to any fictitious deep structure or to any transformations; we do not have to order these meanings in any way, and we do not make the mistake of deriving the participial interpretation from the gerundival one, or vice versa. As Figure 4 shows, both ST_1 and ST_2 can be linked to the sentence *visiting relatives can be a nuisance* and the speaker who, after all, does not want deliberately to mislead the hearer, chooses one or the other of the intended meanings. In most instances the hearer will get the right message, since intonation frequently helps in the disambiguation of such sentences (see Dale Woolley's article in this volume).

The two sememic traces are connected to each other in Figure 4 by means of an unordered OR node. ST_1 <u>or</u> ST_2 become activated depending on what the hearer learned from additional information reaching his consciousness coming in from the pragmo-sememic interface; the decision point whether to take ST_1 <u>or</u> ST_2 is reached through the tactic diamond.

Thus the stratificational method is neither like INCANTATION nor like counter-incantation; but it is rather like PREVENTIVE MEDICINE; that is, like the most ideal form of exorcism there is. It cures the illness before it besets the patient. This method can handle everything that structuralism did handle, and additionally, it also handles all the supposedly unmanageable problems of structuralism without any deep structure, ordered rules, or transformations.

Unlike transformational-generative grammarians, stratificationalists have never SEDUCED or INTIMIDATED any one. Nobody has been called 'stupid', 'undeserving of the name of scientist', 'merely this' or 'merely that', just because he happened to be practicing another brand of linguistics. There have, of course, been comparisons in method, as for example in the article 'Replacives Without "Process" ' by David G. Lockwood (in Makkai and Lockwood 1973), but in this, as well as his other writings, the author carefully documents what various scholars did at various points in time; demonstrates and evaluates their methods by a method (the relational network complexity count) which is 'publicly verifiable' (as Twaddell demands rightfully of science). Yes, stratificationalist writers (including myself) HAVE interfered with the consciousness of others, but never without their approval and never without a

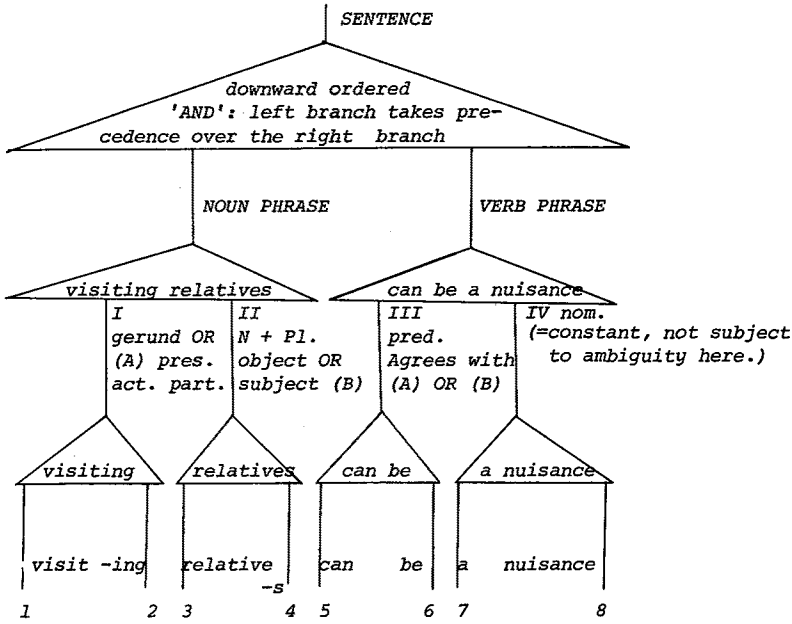

FIGURE 1.

A simplified relational network analysis of the 'surface structure' of the ambiguous sentence *visiting relatives can be a nuisance.* The Lexo-Morphemic constituents 1-8 are ORDERED, that is, we read them off from left to right. (In ordered 'AND' nodes the lines do not come out from a triangle at the same point, they are separated. For an UNORDERED 'AND', where the lines start at the same point, see Figures 2, 3, and 4.) The status of the higher constituents I, II, III, and IV, which depends on the SEMEMIC TRACES here realized in NEUTRALIZATION, is the source of the ambiguity (here specifically I,II, and III with IV the 'identical information' pertaining to both interpretations), if the sentence is encountered out of context. Transformationalist criticism of the Immediate Constituent analysis is pertinent to the extent that if a sentence of this type if encountered out of context, the IC analysis, without some systematic representation of the meaning to fall back on, is indeed not telling us whether to take the 'present active participial' or the 'gerundival' interpretation.
If 'scientific' means strictly 'overtly manifest', however, as Twaddell would have us believe it, there would be no scientific way to account for the ambiguity. But speakers do recognize the ambiguity and the two meanings CAN be diagrammed in the stratificational framework. We then relate the IC analysis to two static structures, Sememic Trace $(ST)_1$ and ST_2 without claiming that the one 'derives' from the other.

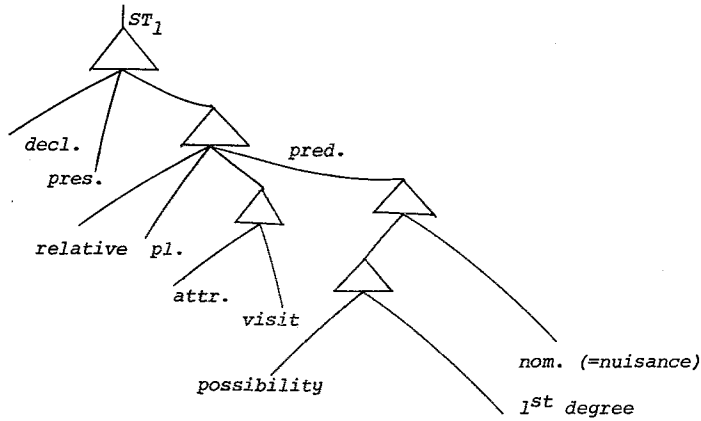

FIGURE 2.

A simplified relational network description of the ST giving rise to the 'present active participle' interpretation with *can* realizing the sememe ^{s}/pl./. Depending on what content sememes are used (e.g., *planes, pennies, cookies, children* instead of *relatives,* and *fly, shine,* etc., instead of *visit,* ST_1 will account for Chomsky's sentence *flying planes can be dangerous* as well as a number of other similar sentences cast in the same mold, such as *shining pennies can be a joy, visiting children can be neat, walking dogs can be attractive,* and so on. Some outcomes will be nonsensical, e.g., *flying relatives can be a nuisance, visiting pennies can be neat, shining relatives can be a bore,* etc. None of these are 'ungrammatical', their illformedness rests in the pragmatic-cognitive incompatibility of the concepts involved. The meaning ^{s}/possibility/ in conjunction with the meaning ^{s}/1st degree/ gives rise to the lexeme ^{l}/*can*/. In conjunction with the meaning ^{s}/2nd degree/ it would give rise to ^{l}/*may*/, and with ^{s}/3rd degree/ to ^{l}/*might*/. A morpheme ^{m}/*may*/ and a morpheme ^{m}/*past*/ yielding ^{m}/*might*/ is not appropriate in a sememic trace. *Will have been, would have been, can be, could be, should be, would be, ought to be, seems to have been,*etc. are all different SEMEMIC PROPOSITIONS indicating different degrees of likelihood, mode, tense, degree of obligation, etc. They are accidentally realized by 'auxiliaries' but this is not a sememic but a lexotactic fact of the English language.

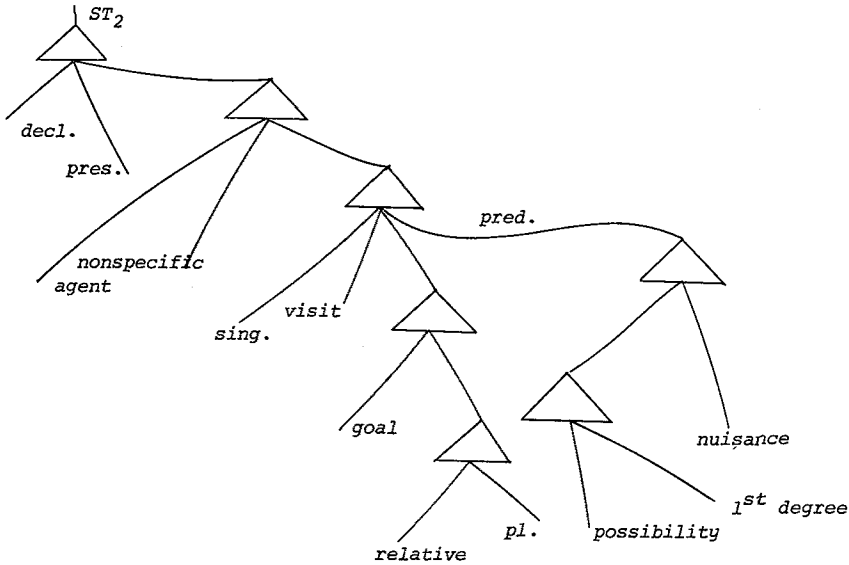

FIGURE 3.

A simplified relational network description of the ST giving rise to the 'gerundival interpretation' with *can* (unlike in ST_1) realizing the ^{s}/*sing.*/. Whereas ST_1 had 5 unordered 'AND' nodes, ST_2 has 7; whereas ST_1 had the concept of plurality, ST_2 has the concept of singularity in construction with the 1st degree of possibility, the ^{1}/*be able to*/, whose morphologically impoverished synonym *can*, due to its history, has lost the ability to show overt difference in singular versus plural. If the ^{1}/*be able to*/ were to be used, the sentences would have to read
1. *Visiting relatives are able to be a nuisance,* versus
2. *Visiting relatives is able to be a nuisance,* and the sememic difference would overtly surface. ST_1 shows that a predication is made of something we attribute to the relatives, i.e.,the fact they visit, ST_2 shows that a predication is made of a nonspecific agent's doing something to some goal (i.e., some one visits relatives, or some one flies planes, etc.). This nonspecific agent is not the 'Deep Structure Dummy' SOME ONE later to be 'deleted'; it is simply realized as 0 on the lexemic stratum with the action sememe realized as the gerund that can carry a direct object.

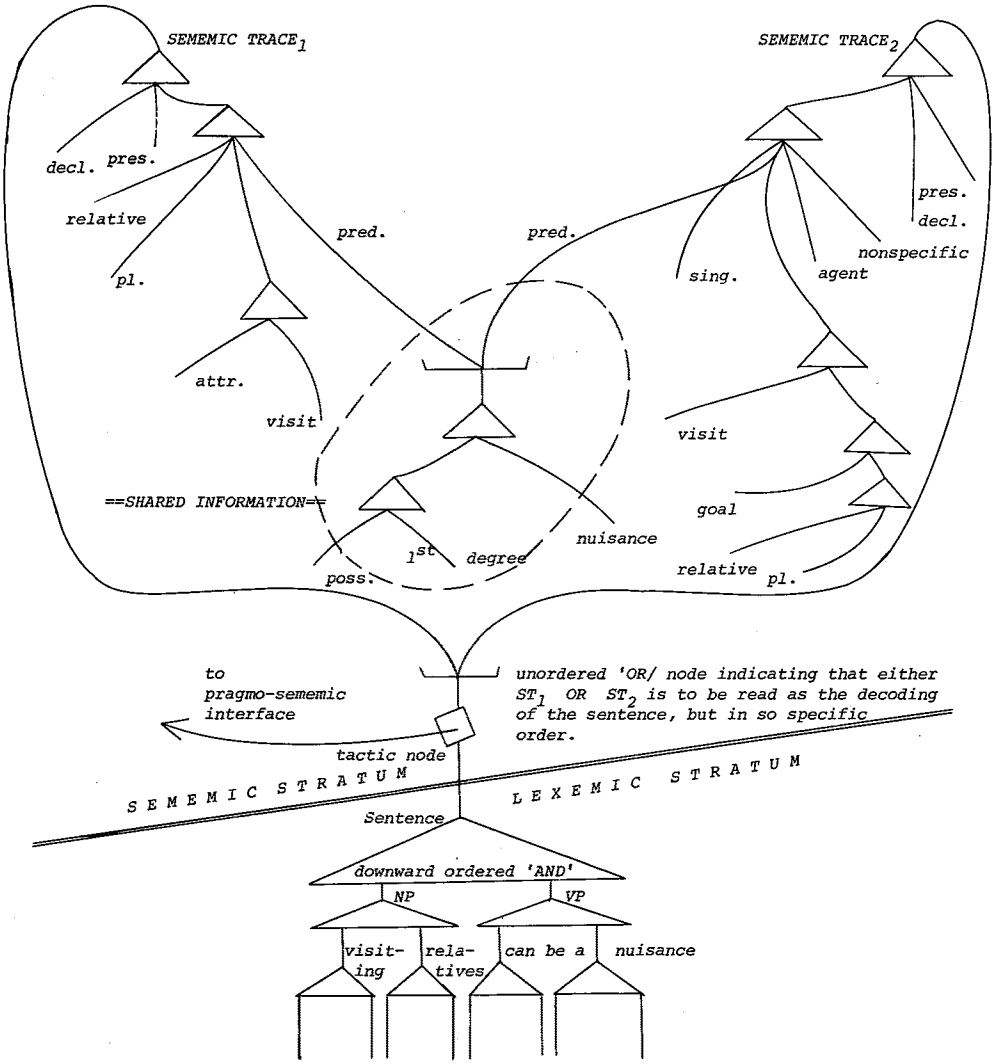

FIGURE 4.

This Figure builds the IC analysis (Figure 1) and its two interpretations (ST_1 and ST_2) together showing the shared information (=*can be a nuisance*) eliminating the need for 'deep structure' and 'transformations'.

clear intention of benefiting them in the process. If I may say what I think is happening, it is this: We INFORM, and do not DISINFORM.

7. In his book *The Divine Animal* (1969) Roger W. Wescott speaks about the 'released state of consciousness'. In this context a released state of consciousness would mean primarily in open mind; coupled with a temperament that is not selling anything and is not buying anything, and especially not under panic pressure, but one that contemplates, evaluates, and compares.

Madison Avenue advertising, 'motivational research', paradigm-mongering, rescuing linguistics from the 'inept clutches of the Neo-Bloomfieldians' (cf. Lamb's review of Chomsky's *Aspects,* Lamb 1967) --- all of these phenomena belong to the TECHNOCENTRIC AGE which, at long last, is coming to a grinding halt. What lies ahead, some of us hope, is the BIOCENTRIC AGE, which might be followed by the PSYCHOCENTRIC AGE. These two could be characterized together as the ECOLOGICAL AGE. (I do not mean by 'ecological age' the ill-argued attempts of left-wing rabel-rousers to shut down the American industry on the one hand, while getting high on drugs on the other and singing the song 'Aquarius' in dimly lit nightclubs. That is the PARODY of the ecological age. But as it almost always happens, the parody has a tendency to manifest BEFORE the real thing that it is a parody of. In traditional religious terminology one could express this true observation by saying that God has a nice plan for mankind, but the Devil steals it hastily and performs a negative sneak-preview of it well in advance, so as to disenchant everybody by the time the real thing is supposed to happen. Thus moral man's task is to remain concerned about one's fellow men DESPITE socialism and communism; to study reace-relations scientifically and objectively DESPITE Hitler, the Ku-Kux-Klan, and forced busing; to remain a free spirit DESPITE fossilized establishment religions, and, if one happens to be an American, as I am by choice rather than by birth, to remain a good American DESPITE Madison Avenue, and its byproducts, such as unnecessary wars, unnuecessary public relations spending, Watergate, etc.)

A released state of consciousness implies a game-free attitude (in the sense of Berne 1966). Chomsky's and Postal's attitudes (especially in the earlier days) are characterizable in transactional terms as I'M OK YOU'RE NOT OK - - - called by Harris (the physician, not the linguist) the typical 'criminal position'. When two outmoded structuralists compare notes on the 'Chomskyan Revolution' on a misery-loves-company basis, they are playing I'M NOT OK AND YOU'RE NOT OK EITHER. A structuralist seeking re-education from a transformationalist or a stratificationalist is endulging himself in a game of I'M NOT OK, YOU'RE OK: He is a beggar for a few positive strokes, such as recognition in terms of publication, or a less negative review, etc.

I hope that the stratificational position is not a counter I'M OK YOU'RE NOT OK, but rather the mature adult-to-adult I'M OK YOU'RE OK position. We have never proselytized or sought converts. We do not advertise and we do not promote one another as tribal kinsmen or as members of a mafia. We research the facts of human speech and diagram it as best we can. The results are there for every interested linguist to see for himself. He can take it or leave it.

Stratificational grammar, to put it in other words, is almost as if it were a new and more efficient can opener which is NOT compared to your mistress; is NOT aligned with your masculine inferiority or superiority, or your feminine inferiority or superiority. We do not call you a reactionary if you do not follow stratificationalism; though we may point out that it is a pity that you would not avail yourself of what we have to offer. We are trying to show you a new can opener with a small instruction manual showing you how to open cans. Even if *opening cans can be unassuming.*

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AFTERWORD

The diagrams in this article are not the only way to describe the 'ambiguity' of the cited sentences. It can be shown, in fact, that *visiting relatives can be a nuisance*$_1$ and *visiting relatives can be a nuisance*$_2$ are different not just sememically, but already on the LEXEMIC level. This involves the presentation of two LEXEMIC TRACES intermediate between the SEMEMIC TRACES and the morphemic constituency diagram. That is, needless to say, the more sophisticated treatment. I will show in a future study why the positing of such intermediate lexemic structures actually simplifies the description. I have kept the technicalities of the matter to a bare minimum here since this article was NOT intended to be a stratificational treatise, but a comparison of Madison Avenue advertising and the channelling of linguistic thought along certain political lines. (I am indebted to Professor Sydney M. Lamb for suggesting the intermediate lexotactic structures. The responsibility for this particular oversimplified analysis is strictly my own.)

IV

Philology and the Comparative Method

A Set of Postulates for Comparative Linguistics

Carleton T. Hodge
Indiana University

The following postulates are a tentative and partial formulation of the principles of language change and genetic relationship. A postulate may be accompanied by one or more explications (E....) and/or one or more comments (C....). The present version does not cover all of the areas treated by Bloomfield's postulates (VIII Historical Linguistics; 1926). Further explication of these postulates is planned in the form of quotations (Q....) from scholars in the field and exemplification (G....[e.g.]). As noted, this set of postulates is not complete; more must and will be written.

POSTULATES

1	P1	Language is a set of arbitrary symbols.
2	P2	The use of more than one symbol for the same referent constitutes variation.
3	P3	Variation is a feature of all observable languages.
4	P4	The innovative use of a symbol as a variable and the subsequent dominant or exclusive use of that symbol vis-à-vis other symbols for the same referent constitutes change.
	E4.1	Referent is here understood in the sense of any functional unit as well as any other definable units, such as sememes.
5	P5	Change is a constant and normal occurrence in all languages.
	E5.1	All languages now spoken and observed contain variables.
	E5.2	All languages known from two points in time exhibit variation and with sufficient time between the two points exhibit change.
	E5.3	It may reasonably be concluded that all languages which have been observed were or are in a state of change.

	C5.1	The description of any language must allow for the creation of structures beyond those commonly accepted at the time of description, in order to accommodate this factor of change.
	C5.2	There are at present no analytic procedures for determining the direction of change from a synchronic study.
	C5.3	Linguistic change may be motivated either mechanically or by speaker purpose. 'Mechanical' motivation is that which results from the physiology of speech production and/or the acoustics of speech. 'Speaker purpose' includes both innovation and imitation of innovation.
6	P6	Language change occurs in all areas of linguistic structure.
	E6.1	Change may occur within a single area of linguistic structure, may involve several areas simultaneously or may involve several areas sequentially.
	E6.2	The assumption that there are language universals implies that there are aspects of language which do not change.
	C6.1	Historians of language and comparative linguistics have recognized the fact that all things may change in a language. It is, however, equally true that all things do not change at the same time.
7	P7	The amount of change which occurs in any given language at any given time varies.
	E7.1	Areas of linguistic structure differ in their quantifiability, given present knowledge.
8	P8	The amount of change which occurs in any given language at any given time does not usually impede communication between speakers of this language.
	E8.1	'Usually' here excludes deliberate change for the purpose of impeding communication between certain members of the speech group.
	E8.2	A statistically low amount of change can be said to be 'normal'.

	E8.3	As change normally affects only part of the language, much of the language remains unchanged from one historically attested time to another.
9	P9	An unusual amount of linguistic change may lead to pidginization or total loss of the language.
	C9.1	'Usual' and 'unusual' are not happy terms. Pidginization is a normal, natural process, as is loss of a language. It would appear to the linguistic observer, however, that fewer languages are lost or pidginized than remain with lesser changes.
10	P10	Linguistic changes are patterned.
	E10.1	Patterned means that such changes occur in sets.
	E10.2	A discernible pattern may be observed either in the conditions under which a change or changes take place, in the results of such change, or in both.
	C10.1	The statement 'most linguistic changes are patterned' is demonstrable in our present state of knowledge. To claim that all such changes are patterned may go beyond present demonstrability.
11	P11	Changes differ not only in kind and extent, but in the time involved to effect them.
	C11.1	Lexical substitution may be very rapid, while the replacement of paradigmatic morphologic sets may take hundreds of years.
12	P12	Effects of change vary in the lengths of time over which they are observable.
	E12.1	Morphs having form/meaning similarity are at present those items of structure which appear to be identifiably derivative through time for the longest periods.
	E12.2	Patterned sound correspondences based on such morphs are the factors observable over the next longest periods of time.

E12.3 Combinations of morphs retain recognizable patterned shapes for less time than either lexica or sound correspondences based on lexica.

C12.1 A morph which is identifiable through time is a chronomorph.

C12.2 The relative time depths are based both on observation and inference. The lesser time depth of at least some morphology is observable. The claim that morphs are derivative from a common source even where sound correspondences have not been established is inferred.

C12.3 There is some evidence that morph usage affects morph survival. Formative elements, for example, appear to survive longer than the roots they accompany.

C12.4 E12.1-3 may be put in a slightly different way. More chronomorphs survive as recognizable units than do bound combinations of chronomorphs. The implications are that lexical items (i.e., vocabulary) and isolable morphologic units are the best source of evidence for remote relationships. Morphology--that is, fixed combinations of morphs--has less time depth.

13 P13 Under like conditions phonological changes in a given language occur with extreme regularity.

E13.1 'Under like conditions' covers a large number of variable factors, including phonological environment and style of speech.

E13.2 Exceptions, that is, sounds which do not in similar environments follow the regular pattern, are the result of other conditioning factors.

C13.1 It is necessary to establish criteria for determining which variants are the result of the regular changes and which form exceptions.

14 P14 In a given speech community there will be one or more focal points the changes in which tend to be adopted by the area dominated by the focal point.

E14.1 The immediate result of such linguistic dominance is the formation of dialects.

E14.2 A speech area formed of different dialects no one of which is dominant over the whole is a dialect cluster.

15 P15 A given language (speech-area, dialect cluster) may in the course of time develop mutually unintelligible varieties.

E15.1 A group of speakers which separates into smaller groups with lessening or no communication between them develops separate languages.

E15.2 Such [generally separated] mutually unintelligible varieties derived from a common source are called 'related languages'. This relationship from a common source is called 'genetic' relationship.

E15.3 The common source of genetically related languages is known as the proto-language.

E15.4 This postulate is stated as a possibility rather than a necessary consequence as it is conceivable that a language never develop such mutual unintelligible varieties.

16 P16 Languages which derive from a common source may retain sufficient structured material (reflexes of patterned change) for their relationship to be determined.

E16.1 Observation of languages retaining patterned correspondences does not preclude the possibility that other languages derived from a common source may lose so much of these correspondences that establishment of their relationship is no longer possible.

E16.2 When change occurs, it is subject to analysis and comparison only if the resulting linguistic structures retain sufficient evidence for an identification to be made.

C16.1 This postulate is the basis for comparative linguistics. As the patterning of phonologic restructuring is the aspect of linguistic change most amenable to statement by rule (cf. P13), it is at present the most reliable guide to the determination of genetic relationship among languages.

17 P17 The greater the time depth of separation the more probable it is that languages derived from the same source will have less in common.

C17.1 Since according to P11 Changes differ not only in kind but in the time involved to effect them and P12 Effects of change vary in the lengths of time over which they are observable, it follows that the 'sufficient structured material' (P16) necessary for determining linguistic relationships will be of different kind and in different quantity depending on the length of time since divergence was effected. That is, different areas of linguistic structure will be of varying values in relation to time depth.

Those areas of structure with the greatest retention will serve to indicate the most remote discernible relationships. Those areas with the shortest period of retention will serve as indications of the closest relationships. This assumes the cumulative use of all evidences of relationship wherever these are found.

C17.2 This assumption is not yet tested in all areas. Lexicostatistics assumes a regular rate of change for replacement of core vocabulary. The formula for rate of change is disputed but the general principle of greater difference over a greater period of time is usually assumed.

C17.3 It is also generally assumed that greater time of separation will also mean greater divergence in phonology and morphology.

C17.4 Divergence in syntax is clearly present but has not yet been quantified.

C17.5 Divergence in semantics needs definition even to be considered. One may assume: 1) basic semantic motifs remain constant 2) the association of these motifs with phonologic structures shifts 3) the shifts in association also involve different combinations of semantic motifs in relation to phonologic segments 4) 'Bundles' of semantic motifs may remain together in related languages even though the phonologic structures with which they are associated have changed.

18 P18 Languages which still retain sufficient structured material inherited from the time when they were part of the same speech area may be demonstrated to be related.

E18.1 At the present time the only structured material which is sufficiently amenable to accurate statement and hence usable for this purpose is patterned sound correspondences.

E18.2 Other aspects of linguistic structure should be under constant observation to see whether they may be defined with sufficient rigor to serve as prime determiners of relationship.

19 P19 Where data are sufficient for the establishment of sound correspondences, they are sufficient for the partial reconstruction of the language from which the related languages are derived.

E19.1 Different areas of structure are reconstructable, though all only partially.

E19.2 The most common unit of reconstruction is the chronomorph, though sequences of chronomorphs are often feasible.

E19.3 Reconstruction aims at phonologic probability.

E19.4 The reconstructed segments of the proto-language have implications for the history of each derived language between the time at which the proto-language was spoken and the time at which the derived language was first attested.

20 P20 Within a group of languages all of which share inherited characteristics, any languages of this group which share characteristics not possessed by the rest form a subset more closely related to each other than to the rest.

E20.1 These more restricted characteristics may be 1) inherited from the proto-language or 2) the result of changes which occurred after the subset of languages left the proto-language.

E20.2 Such shared innovations (2) are the prime criteria for the establishment of more closely related groups within a larger group.

21 P21 All languages operate with a semantic reassignment pattern such that there is a tendency toward the association of either a single motif (or very few motifs) with a recognizably separable (if not separate) form.

	E21.1	The operation of this reassignment of semantic motifs is the major factor in linguistic continuity.
	C21.1	If we are looking for universals--factors which stay the same throughout languages and language histories--the most obvious area in which to look is the operation of this semantic reassignment.
22	P22	Cultural value is often attached to the preservation of linguistic structures, resulting in the simultaneous use of older and newer forms.
23	P23	Older structures are often partially preserved in fixed or near-fixed forms, long after the original structures have passed from active use.

THE ACRONYM AND ITS CONGENERS

JOHN ALGEO

University of Georgia

The initial meeting of LACUS is an appropriate time to reflect on the initials of the organization and to meditate on the fact that LACUS is, at least as far as I know, the first linguistic organization to be named with an apposite acronym. As such, it is clearly at the fore tip of the lexical evolution of English and is also the inheritor of an ancient tradition. This paper is, then, an examination of the kind of name LACUS bears and was written for its birth day.

1. The Use of the Acronym

The use of acronyms or of acronym-like words has a long history. There was early use of initial-letter abbreviations as labels, for instance, the Roman SPQR (for Senatus Populusque Romanus) and INRI (for Jesus Nazarenus Rex Judaeorum). The Christian monogram IHS, which originated as a partial transliteration of the Greek abbreviation for the name Iesous, was taken as an acronym for Latin Jesus hominum salvator or in hoc signo (vinces) or in hac (crux) salus. To make alternative interpretations of acronyms is part of the tradition of their use. After it had been adopted from Roman bureaucracy by Christian ecclesiastics, INRI was given an alchemical interpretation as igne natura renovatur integra; similarly, but in a profane register, MDUSA for Medical Department United States Army was given a black-comedy interpretation during the first World War as many die, you shall also (Lighter 1972), and R&R from rest and recreation (or recuperation) was interpreted during the Korean hostilities as rape and restitution.

It is remarkable how often the same kinds of changes have been wrought upon acronyms. The use of the initial letters of a phrase to spell out a word whose meaning is appropriate to that of the original phrase is at least as old as the primitive Christian interpretation of ichthus 'fish' as an acronym for Iesous Christos, Theou Uios, Soter 'Jesus Christ, God's Son, Savior,' which partially explains the image of the fish in ecclesiastical art. The use of such apposite acronyms has lately enjoyed a resurgence in popularity, with organizational names like Care, Ujoin, Hep, SOS, and a host of similar forms, but it is merely the revival of a very old practice.

The Christian use of acronyms like ichthus, which coincide with pre-existent words or, at least, are pronounceable as a word, doubtless had its roots in Jewish practice, in which the formation of such acronyms has been common. For example, from the consonantal initials of the names and titles of medieval rabbis, acronymic personal names were formed, such as Rashi from Rabbi Solomon ben Isaac, Rambam from Rabbi Moses ben Maimon, Hida from Hayyim Joseph David Azulai, Ramak from Rabbi Moses Kohen, and Adam from Abraham Dob Michaelischker. The acronym tenach from Torah 'Pentateuch,' Nebi'im 'Prophets,' and Kethubim 'Hagiographa' is the usual Hebrew name for the Bible.

Kabbalistic interpretation of the Torah used several techniques, of which one, namely notarikon, is a form of acronymic exegesis. In it, the letters of a word are taken as the initials of a phrase that reveals the hidden meaning of the word. For example, the word MILH 'circumcision' is formed from the initials of the Hebrew phrase MI IOLH LNV HSHMILH 'Who shall go up for us to heaven?' (From Deuteronomy 30:12); and, in another variation of the procedure, the final letters of the same phrase form the ineffable name IHVH. Thus it is acronymically demonstrated that the answer to Deuteronomy's question is that the circumcised will reach God.

It is perhaps not accidental, in view of the importance of acronyms in Kabbalistic exegesis, that the English word cabal, derived from a Franco-Latin form of Hebrew Kabbalah, was given an acronymous interpretation in the seventeenth century. During the reign of Charles II, the term was applied to a group within the Privy Council, officially known as the Committee for Foreign Affairs. Five of the prominent members of that committee were Clifford, Arlington, Buckingham, Ashley (the Earl of Shaftesbury), and Lauderdale, whose initials chanced to spell cabal. Because those men were instrumental in arranging the treaty of 1672 between England and Catholic France for war against Holland and thus were suspected of promoting popery at home and abroad, the term cabal was associated with intrigue and usurpation, which have become its dominant sense.

In the early years of the eighteenth century, Jonathan Swift made use of initial abbreviations in the "little language" of his letters to "Stella," for instance, pdfr for poor dear foolish rogue in reference to himself or ppt for poor pretty thing. There is some irony in the fact that Swift, who in his public life opposed degenerate shortenings like mob from mobile vulgus, should have, in his private life, been a devotee of the playful abbreviation.

In examining the history of abbreviations in English, we often cannot be sure when a written abbreviation ceased to be read aloud as the corresponding full form and began to be pronounced as the abbreviated spelling suggests, by letter names or otherwise. For example, manuscript has been abbreviated MS since the seventeenth century, but it was not until 1818, when Byron used the latter form in a poem ("The unfledged MS. authors come"), that we can be sure from the meter that the intended pronunciation was "em ess." Byron likewise provides our earliest citation for the popular acronym for member of parliament: "All hail, M.P.! from whose infernal brain / Thin-sheeted phantoms glide."

In the United States, for several years after the summer of 1838 in Boston, there was a fad for using alphabetical abbreviations, often of misspelled expressions. One of the earliest of such abbreviations was OW for oll wright, but the most enduring by far has been OK for oll korrect (Read 1963a), an acronym whose popularity was reinforced when it was adopted as a political slogan and reinterpreted as Old Kinderhook, with reference to the birthplace of Martin Van Buren (Read 1963b). The origin and subsequent history of this most successful of all acronyms has been traced by Allen Walker Read (in the works just cited and in subsequent articles 1964a, b, c).

Another fad for the use of acronyms, including those pronounced by normal rules of orthoepy, began in the early years of the twentieth century. Especially common were trade names and governmental terms, of which Dora for Defence of Realm Act was one of the best known in England. In the United States the period of blossoming came during the New Deal administration, with its alphabet soup of federal agencies. During the second World War and since, the growth of acronyms has been unabated, so that Margaret M. Bryant's remark (1962; 1st ed., 1948: 312) that "a source of new words of very minor but perhaps growing importance consists of the initials or initial syllables of words" has been prophetic. Today acronymy is an abundant source for new words and especially for new names of organizations. The process, however, is certainly not a new one.

2. The Study of the Acronym

With the increased popularity of the acronym in the twentieth century has come also a scholarly recognition of the type and an attempt to describe it and to relate it to other types of word making.

Although there were earlier notices (Haldeman 1877: 11), in 1913 Louise Pound devoted a section of her "Word-Coinage and Modern Trade-Names" to "trade terms built from initials," instancing the Reo automobile (for R. E. Olds), Sebco (for Star Expansion and Bolt Co.), Pebeco tooth-powder (for P. Beiersdorf and Co.), and Reeco (for Rider-Ericsson Co.). In 1915, Percy W. Long published a note on "Semi-Secret Abbreviations," which included some acronyms in its list of examples. By 1937, Herbert Koziol was able to discuss the type, which he called Initialwörter, in his Handbuch der Englischen Wortbildungslehre.

E. Kruisinga (1942), in an observation that could no longer be made, noted that "the practice of letter-words [by which he meant acronyms pronounced orthoepically] seems to be infrequent in English, which generally enumerates the initials only, as in S.P.G." And in volume 6 of A Modern English Grammar, completed under unhappy circumstances also in 1942, Jespersen noted the fashionable use of acronyms pronounced by normal orthoepy during the first World War and added, on the last page of the last volume he was to finish: "Whether similar words have come into existence during the second

world war I am cut off from knowing." As he doubtless suspected, they had and were recorded in a large number of articles written in the years during and immediately following the war (of which the following are a sample of those in American Speech: US Army 1941, Wilson 1942, Hornstein 1945, Army and Navy Journal 1945, Shafer 1945, Berger 1945, Dunlap 1945, Fleece 1946, and Riordan 1947).

The term acronym was recorded by I. Willis Russell in "Among the New Words" in 1948 as having been first used five years earlier. The term quickly became the accepted designation for a word made from the initials of a phrase or compound and was so used in general discussions of English word formation by scholars like Potter (1950), Pyles (1952), Robertson and Cassidy (1954), and thereafter quite generally.

Dictionaries of abbreviations have perforce included acronyms. They have been published from 1913, the year Louise Pound called scholarly attention to the forms, to the present day, becoming ever more abundant in number and size (Rogers 1913; Stephenson 1943; Shankle 1945; Allen 1946; Partridge 1949; Schwartz 1955; De Sola 1967; Kleiner 1971). The queen of such works, which is devoted explicitly to the word type, is the Acronyms and Initialisms Dictionary (AID), first published in 1960, with subsequent, revised and greatly expanded, editions in 1965, 1970, and 1973 (edited by Ellen T. Crowley and Robert C. Thomas). In thirteen years it has grown from a listing of 12,000 entries to one of 103,000 entries, without sign of let-up. Periodic supplements record new acronyms (the first supplement to the fourth edition appearing in 1974).

Detailed scholarly attention to the acronym was begun by S. V. Baum in his 1955 "From AWOL to VEEP: The Growth and Specialization of the Acronym" and extended through several shorter articles (1956, 1957) to his analysis of the various kinds of acronyms and acronym-like words in "The Acronym, Pure and Impure" (1962). The last article called attention to the diverse ways in which the term acronym had come to be used and tried to bring order out of confusion. Yakov Malkiel (1968) discussed acronyms as one of the "Secondary Uses of Letters in Language," calling attention to the fact that writing is a form of language expression that has had a good deal of influence on the lexicon. Louis G. Heller and James Macris (1968) have sought to place acronymy within the framework of a general "Typology of Shortening Devices," and Algeo (1974) has tried to place it within an even more general "Taxonomy of Word Making."

The foregoing studies are largely diachronic in focus. There have been two noteworthy treatments of the acronym from the standpoint of synchronic word derivation. Rulon Wells (1956) considered the morphemic relationship between acronyms and their etyma and concluded that they are different morphemes, though "somehow related in a grammatical way to each other." In Saussurian terms, he regarded Nato and North Atlantic Treaty Organization as members of the same associative, but nonparadigmatic, set. Makkai, in

"Acronymy in English" (1972), has taken up Wells's analysis by showing how an acronym and its corresponding full form can be related to each other within a stratificational grammar. He considers the cognitive basis on which speakers form and understand such items.

3. The Definition of the Acronym

As the use of acronyms has grown and the scholarship on the word type has expanded, so has the associated terminology. Indeed, central to most of the scholarship on the form has been an effort to define the type and to clarify the use of the term acronym.

The earliest recorded use of acronym was in 1943, in American Notes and Queries (as cited by Russell 1948): "Your correspondent who asks about words made up of the initial letters or syllables of other words may be interested in knowing that I have seen such words called by the name acronym, which is useful, and clear to anyone who knows a little Greek." Although that citation implies that its use of the term is not an innovation but rather a report of prior use, no earlier example has been attested, and 1943 is generally accepted as the year in which the term originated. The term acronym has, indeed, proved useful, but it has not been wholly clear, regardless of whether one knows a little Greek, as can be seen from the disagreements about the term's definition and application.

3.1 The Pronunciation of Acronyms

One area of disagreement concerns the manner of pronouncing an acronym. When an expression is represented by a series of initials, there are three major ways of pronouncing them (Hargreaves 1957 considers the question from the standpoint of the foreign learner of English). First, they may be taken simply as a representation for the full expression, which is used as the pronunciation of the abbreviation. Thus, D.T., ms., and W.Va. would usually be pronounced as the corresponding full forms, daylight time, manuscript, and West Virginia. (Hereafter such forms will be followed by abbreviatory periods and may be either capitals or lower case.) Second, they may be read off as letters. Thus DTs, MP, and US, although standing for delirium tremens, military police, and United States, are commonly pronounced "dee tees," "em pee," and "yu ess." (Here such forms will be written in unpointed capitals.) Third, they may be pronounced by taking the letters in their normal orthoepic value in the English spelling system. Thus, Zip (code), laser, and Unesco, although representing zone improvement plan, light amplification by stimulated emission of radiation, and United Nations Educational, Scientific, and Cultural Organization, are almost invariably pronounced as one-, two-, and three-syllable words, respectively. (Here such forms will be written in unpointed lower case, except for the initial capital of proper names.) A single full form may, of course, have all three shortenings. Thus, the military term absent without official leave can be written A.W.O.L. or a.w.o.l., which continue to be pronounced like the full expression; AWOL, pronounced "ay double-u oh ell"; or awol, pronounced "ay-wall."

Purely written abbreviations of the A.W.O.L. or ms. type have seldom been considered to be acronyms, although that fact has often been implied rather than made explicit by definitions of the term. To be an acronym, it is not enough for a form to be represented by a shortened spelling; the shortened spelling must be pronounced in a manner distinct from that of the full form. The pronunciation need not be shorter; WW pronounced "double-u double-u" is six syllables, whereas world war is only two. There is, however, an unspoken consensus that merely written abbreviations are not acronyms.

There is likewise general agreement, although also often inexplicit, that abbreviations of the awol or laser type, pronounced by orthoepic rules, are properly acronyms. The use of the term word in definitions and discussions of such forms may be intended to imply orthoepic pronunciations.

The disagreement comes about forms that are read off as letters, like AWOL or MP. Marckwardt (1958:95-97) and Pyles and Algeo (1970:111-12) explicitly treat such forms as a type of acronym. Robbins (1951:67) and Baum (1962: 48) just as explicitly exclude them from the class of acronyms, regarding them instead as mere abbreviations. There are, however, several reasons why it is better to regard alphabetically pronounced letters as a type of acronym. First, like the orthoepically pronounced acronym, and unlike the purely written abbreviation, they have a pronunciation different from that of the corresponding full form. Second, the alphabetic pronunciation is sometimes spelled out, as in emcee, okay, kayo, Seabees, Esso, essohbee, and peejays; and then the word loses much of its character as an abbreviation. Third, some forms have a pronunciation that combines alphabetic pronunciation with orthoepic rules, for example veep (for vice president), jeep (from GP, for general purpose), beep (from bedpan), and ROTC pronounced "Rotsy." Fourth, although most definitions of the acronym do not explicitly consider its pronunciation, they often include among their examples forms like MP; thus, general opinion is implicitly in favor of counting such forms as acronyms.

Several other pronunciation features of acronyms are worth noting. In some cases, an acronym cannot be pronounced according to orthoepic rules for lack of vowel symbols at appropriate places. Such acronyms may nevertheless be pronounced by supplying vowels. Examples are SNCC for Student Nonviolent (later National) Coordinating Committee pronounced "snick" and SCLC for Southern Christian Leadership Conference pronounced analogously, though less happily, "slick." It seems probable that Lem for Lunar Excursion Module is in fact an expansion of LM for Lunar Module based on the pronunciation "lem" with the vowel from the letter name M. WRNS for Women's Royal Naval Service was given an orthoepic pronunciation by inserting the vowel from the letter name N; then by backformation a new singular Wren was created for a member of the Service (with a pun on bird in the British slang sense doubtless hovering over the form). ICWPA (International Civil War Prisoners' Aid) pronounced

"icky woppa" was cited by Wölcken (1957: 328), and SECDR (South Eastern Chatham and Dover Railway) pronounced "Seceder" by Bergström (1906: 67).

Another pronunciation feature is the use of descriptive phrases for acronyms that consist of a succession of identical letters: Tri-Delt for the Delta Delta Delta sorority, Triple A for Automobile Association of America, Four Cs for Conference on College Composition and Communication, 3M for Minnesota Mining and Manufacturing (Company), and so on. Finally, when alphabetic pronunciation is used, it can be drawn from alphabets other than the traditional English. Thus, the signal alphabet may be used to render AM and PM as ack emma and pip emma and has been so regularly used for AA (anti-aircraft) that ack ack is the expected term. Members of the Phi Gamma fraternity are known bialphabetically as Fijis, with a pun on the Melanesian people.

3.2 The Etyma of Acronyms

A minor, although troublesome, question is whether an acronym must be based on an etymon consisting of two or more words in a compound or phrase, or whether it can be derived from a single word. NATO and NG (from North Atlantic Treaty Organization and no good) would generally be accepted as acronyms, but there is question about ad and PJs (from the single words advertisement and pajamas).

Pearce's (1947: 6) definition, "a word formed from the initial letters of other words," was the first to make explicit the limitation that an acronym's etymon must involve more than one word. Robertson and Cassidy (1954: 214) further specified that the words of the etymon must compose a phrase, and Webster's Third (1961) broadened the requirement in proposing that the letters be drawn from "each of the successive parts or major parts of a compound term." The Acronyms and Initialisms Dictionary (1973: viii) is explicit in regarding "a shortened form of a single word" as an abbreviation, "while acronyms and initialisms are considered to be shortened forms of two or more words or of an entire phrase [emphasis original]." Nevertheless, AID now includes some abbreviations within its scope.

Most definitions of the acronym, whether explicit or implicit, have used criteria like those above. Yet there are problems in their application. For example, is headquarters one word? If so, by the AID criterion, HQ is an abbreviation, not an acronym. On the other hand, since headquarters is clearly a compound, by the W3 criterion, HQ is a legitimate acronym. Under one interpretation of the W3 definition, television and dichlorodiphenyltrichloroethane, although their components are not words, are nevertheless "compound terms" and thus TV and DDT would also qualify as acronyms.

The problem is greater with TB and PJs. The etymology given by Onions (1966) implies that TB was originally an acronym for tubercle bacillus (a sense it still has in technical use) but was popularly reinterpreted as standing for tuberculosis. In current use, the letters of TB are generally taken

as the first two consonants of the latter word, and thus fall under none of the widely accepted definitions of acronym. Similarly, PJs, which is a relatively recent term, is derived from the first two consonants (and the plural ending) of pajamas. The fact that the latter word derives, through Hindi pājāma, from Persian pā 'leg' and jāma 'garment' is doubtless irrelevant to the origin of the current short form and is certainly irrelevant to its use.

Thomas Pyles grouped together all such short forms in his observation (1971; 1st ed., 1964: 287) that "the use of the initial letters of the words in phrases (O.K., Y.M.C.A.), sometimes of syllables (TB, TV, PJs 'pajamas'), as if these were words has long been common." Although his definition is more inclusive than most, Pyles still implicitly excludes from the class of acronyms a series of sequential letters, pronounced either alphabetically (ID for identification card, OB for obstetrics) or orthoepically (prof for professor), as well as spoken shortenings that may be respelled (mike for microphone). On the other hand, Heller and Macris's (1968: 203) proposal that the term acronym be "applied to the unit taken from the beginning of the word, as in ad for advertisement" would qualify ID, OB, prof, and mike as acronyms, but not TB, TV, or possibly HQ, which would be rather, in their terminology, "acromesonyms." Wells (1956) had also used a very broad definition that embraced clipped forms of all kinds.

Although there is not unanimity, the weight of lexicographical and other scholarly opinion is in favor of restricting the definition of acronym to a shortening based on the major parts of a morphemically complex term, or "compound term" as Webster's Third puts it. Thus, HQ, TV, DDT, and TB 'tubercle bacillus' are acronyms, but TB 'tuberculosis,' PJs, ID, OB, prof, ad, and mike are not. That restriction is reasonably descriptive of the most common use of the term.

3.3 The Extent of Shortening

Another question on which there has been some difference of opinion is how extensive the abridgement of the parts may be. Some items are formed from single initial letters (laser from light amplification by stimulated emission of radiation) others include sequences of letters (shoran from short range navigation, KComZ from Korean Communications Zone), and still others combine letter sequences all of which represent syllables (Amvet for American veteran; Satchmo from Satchel Mouth).

As the Acronyms and Initialisms Dictionary puts it, "Acronyms are, strictly speaking, words made from the tips of other words--but the question immediately arises, 'How large a tip?'" AID's own answer to the question, although attributed to "some students of language," who otherwise remain unidentified, is that acronyms are properly made from syllables, either wholly (arbor for arthropod borne) or partially (Bart from Brooklyn Army Terminal). Words formed exclusively from initial letters (AID for

Agency for International Development, or Acronyms and Initialisms Dictionary), although acronym-like, are initialisms. This response to the question is exactly opposite to that preferred by S. V. Baum (1962:49), who calls an item like laser, formed exclusively from single initial letters, "a pure acronym or acronym of the first order." Items that use more than a single letter from a part (radar for radio detecting and ranging) he calls acronyms of the second order; and those composed of initial syllables (minicam for miniature camera) he calls acronyms of the fourth order.

Neither Baum's distinction nor that of AID reflects general practice. The oldest recorded definition of the acronym, from 1943 (Russell 1948), applied the term to "words made up of the initial letters or syllables of other words," and that breadth of application has remained the usual practice.

3.4 Fore Shortening and Hind Shortening

There is a more basic difference of opinion about whether acronyms must be composed of initial elements, or may also include internal and especially final elements. Radar, from the initial parts of radio detecting and ranging, is an acronym; but is racon, from the initial and final parts, respectively, of radar (or radio) beacon, also an acronym?

The earliest view, from 1943, is that the component parts of the acronym must be initial elements of the sources. According to Baum (1962: 49), however, as early as 1950 the Funk and Wagnalls Company applied the term acronym to words like motel or racon, which had earlier been known as portmanteau words, telescope words, or blends. This use he referred to as "acronyms of the third order," and he believed it to be a confusion of terminology, as did Robertson and Cassidy (1954: 214) even earlier. Nevertheless, Mary Reifer, in her Dictionary of New Words (1955), provided that acronyms might be "made up of the first letters of a series of words, or of the beginnings and ends of a series of words" and cited motel as an example of the latter construction. Wells (1956) was even more liberal in citing phone from telephone as example of an acronym.

A. H. Marckwardt (1958: 95-97) developed a taxonomy of word blending with the following categories:

1. fore part plus full word (Amerindian)
2. full word plus hind part (newscast)
3. fore part plus hind part (motel)
4. fore part plus fore part (minicam)
5. initial letters
   a. pronounced orthoepically (Unesco)
   b. pronounced alphabetically (DDT), and perhaps spelled out (Seabees)
   c. pronounced in a combined way (veep)
   d. pronounced descriptively (AAA "triple A")
   e. pronounced with inserted vowels (HFDF "huff duff")

Marckwardt applied the term acronym to categories 3, 4, and 5 (in all its subvarieties). The distinctive characteristic of the term, as he used it, is that an acronym is any word made up exclusively of parts of other words. This is a clear and neat terminological distinction, which has, however, not been widely imitated. Common use of the term continues to apply it to initial bits only, although there is no reason, etymological or other, why an acro- 'tip' could not come from the hind half as well as the fore.

The restriction that acronyms must be composed of the fore parts of morphemes has as a consequence that a form like QT (from quiet, chiefly in the expression on the QT) is disqualified from acronymic status on double grounds: first because its etymon is monomorphemic and second because the component letters are initial and final. It is instead a contraction pronounced alphabetically.

3.5 The Acronym as a Written Form

A characteristic of the acronym that seems never to have been made explicit, although it is implicit in most treatments of the form, is the fact that acronyms are formed in writing and are subsequently pronounced. They are the cultivated flowers of literacy, not the wild growth of speech. The fact that acronyms originate in the orthographic medium of expression rather than the phonological is implicit in all those definitions of the term that begin "a word formed from the initial letters ..." or the like.

Although it is often impossible to tell whether a word formed in recent times originated in speech or writing, occasionally the form will betray its origin. For example, the television term situation comedy has been shortened twice--once in speech to produce sitch com with a pronunciation that preserves the first syllables of the original phonic form and a spelling necessarily changed to match the pronunciation--and once in writing to produce sit com with a spelling that preserves the beginnings of the original graphic forms and a new pronunciation derived from the spelling. When it is possible in a similar fashion to determine from the pronunciation and spelling of acronyms the medium in which they were formed, they invariably prove to have been of orthographic origin.

To say that an acronym was formed originally in writing does not mean, to be sure, that it was created by being set down on paper. It does mean, however, that the form was created through the arrangement, whether physical or mental, of graphic symbols and that the pronunciation of the form was derived from those symbols.

3.6 Two Definitions

With the foregoing matters clarified, it is possible to frame a definition of the acronym that expresses the conditions explicit in most formal definitions or implicit in majority use. No single definition, as must be

clear from the discussion thus far, can encompass all uses of the term, but the following is compatible with the way the term is most often applied:

> An acronym is a word formed orthographically by combining the initial letter or letters of the major parts of a morphemically complex term and pronounced either by letter names or according to orthoepic rules (or less often by a descriptive phrase, with inserted vowels, or in a combination of these ways).

This definition represents majority use, but would exclude many items that have been called acronyms. If, instead, we seek a broad definition that will include as many as possible of the uses of the term, it can be expressed in a simpler way:

> An acronym is a word formed from part of another word or exclusively from parts of other words.

In this use, the term acronym is virtually synonymous with clipped form. Various compromises are also possible, such as Marckwardt's, already referred to, by which an acronym must be derived from two or more original words, but which is otherwise like the broad definition.

The prevailing indecisiveness about the definition of acronym can be clearly seen in the Acronyms and Initialisms Dictionary, which provides in its prefatory matter that an acronym should be derived from more than a single word by combining their initial syllables, but which includes forms like prof (professor), smog (smoke, fog), and QT (quiet). Moreover, AID also uses the term acronym in its prefatory comments to denote any of the entries in the body of the dictionary, thus presupposing the second, most general, definition above.

What is needed is a general taxonomy of word making that will allow the classes that have grown up over the years, independently of one another, to be integrated and defined within a coherent, comprehensive classification (Algeo 1974). Without such a general taxonomy, the definition of individual terms must remain variable and uncertain.

4. The Congeners of the Acronym

In addition to the widely used acronym, a number of other terms have enjoyed some popularity, or at least have been proposed, as alternatives for it or as designations for subclasses and related word types.

Initialism is often used to denote particularly the kind of acronym composed exclusively of the first letters of words (snafu and the like). Louise Pound (1913: 38) wrote of "trade terms built from initials," thus precursing the later technical term, and Koziol (1937: 227) denoted the

whole class of acronyms as Initialwörter. Partridge (1942) cites a 1938 use of initial word, a term also used by Berg (1953: 12), Baum (1957: 73), and Bryant (1962: 226), with Berg specifying that an initial word must be pronounced orthoepically. Robbins (1951: 67) wrote of initial acronyms. (Louise Pound 1956: 9) proposed initialism as a popular equivalent of the learned acronym, but the widespread use of the latter term in the press (for example, Time 1961, 1970) has obviated any need for a more popular alternative. Initialism is now used most often in the sense 'acronym formed from initial letters,' whether pronounced alphabetically or orthoepically. Other terms for the same referent, reported by the Acronyms and Initialisms Dictionary, are abecedism and siglum (Crowley and Thomas 1973: xiv, ix); and earlier terms are acrostic (Haldeman 1877: 11) and protogram (Frank Horace Vizetelly; both cited by Wentworth 1934: 32-33).

Berg (1953: 12), who wished to restrict initial word to an item with orthoepic pronunciation, proposed capital-letter abbreviation for an item like GI with alphabetic pronunciation. The term is not a happy one, however, because such pronunciations are not always written in capitals, as can be seen from the variation between "a.m." and "A.M." and a good many other items.

Kruisinga (1942: 12) used the term letter word for acronyms pronounced orthoepically, as did Marchand (1960: 368), who further specialized the term to initialisms. Graff (1932: xxxviii, 299) had earlier used the term for initialisms. Marchand also used the complementary term syllable word for acronyms like sial (from silicon and aluminium), all of whose parts are syllables; although recognizing their existence, he had no distinctive term for the mixed type (such as radar), consisting partly of syllables and partly of letters, but he used manufactured word as a general term for an acronym. Wells (1956) distinguished between alphabetic acronyms (A for Abraham) and syllabic acronyms (vet for veteran).

Early terms for acronyms in general were alphabetic shortening (Jespersen 1942: 551) and alphabetic designation or alphabetic expression (Riordan 1947: 109, 113). They are seldom if ever encountered nowadays. A related term, alphabetism, is used by Quirk et al. (1972: 1031) to denote the kind of acronym (like COD) that is pronounced as a sequence of letters. Malkiel (1968: 377) uses literation for such a pronunciation. Because of the homophony of a literation with alliteration, the former term is superior in clarity.

Two terms that are used more often in other senses (specifically for blends) have been pressed into service as designations for acronyms. They are portmanteau (Robbins 1951: 67) and telescope word. The latter was reported as 1950 Funk and Wagnalls use by Baum (1962: 49); and it was exemplified by Comsat but not defined in the 1965 second edition of the Acronyms and Initialisms Dictionary (Crowley and Thomas 1973: x); it has

a variant, telescoped name (Potter 1950: 163). Use of these terms for acronyms has never been common.

A practice that has become increasingly popular in recent years, especially for organizational names, is the construction of an acronym that coincides with an already existing word whose meaning is appropriate to the referent (Baum 1955: 108). Relatively mild examples are Spar (from semper paratus, the Coast Guard motto, for a member of the women's reserve), Core (for Congress of Racial Equality), and AID itself (Acronyms and Initialisms Dictionary). We may suspect that such apposite acronyms (as they are called by Baum 1956: 224) or punning acronyms (Makkai 1972: 1) have been constructed with at least half an eye on the words they spell out. In other cases we can be sure that the word played a major role in the construction of the supposedly original name. These prefabricated acronyms, "chosen for their catchy suitability and then fleshed out with more-or-less appropriate words" (Crowley and Thomas 1973: vii), also called reverse acronyms (Pyles 1971: 300), are exemplified by Negro (for National Economic Growth and Reconstruction Organization), Jobs (for Job Opportunities in the Business Sector), Now (for National Organization for Women), and Witch (Women's International Terrorist Conspiracy from Hell).

Such fine flowerings of the acronymic garden, according to Makkai (1972: 8), require for their formation and understanding the "simultaneous awareness of transmodal cognition," fittingly acronymized as Satcog. They are a special kind of double blend, combining the syntagmatic blend of one etymon (Harlem Education Program > HEP) in an associative blending with another etymon (hep 'aware, alert, knowing') or even more than one (help in the Southern and hence black pronunciation "he'p"). In choosing the name for an organization, it seems clear that namers often create both full name and acronym simultaneously so as to assure a happy match, for example, Famous for French-American Mid-Ocean Undersea Study.

One clear sign of the prefabricated acronym is confusion about what the initials are supposed to stand for. Thus, Care has been glossed in one place or another as Cooperative Agency for the Relief of Europe, Committee for American Relief in Europe, Cooperative for American Remittances to Europe, Cooperative for American Remittances (to) Everywhere, Cooperative for American Relief Everywhere, and doubtless in other ways as well. What the letters of Care stand for is of secondary interest; the word they spell has priority of importance and quite likely of chronology too.

Occasionally acronyms turn out to be inappropriate in their associations (UN is a well-known example) or at best wryly appropriate (wasp for White Anglo-Saxon protestant and CREEP for Committee to Re-Elect the President). It is inevitable that this sort of acronym should give rise to sportive fictions. The Baltimore Sun's suggestion that a proposed Business and Labor department of government should be known acronymously as BLAB is Menckenian in spirit. Similarly playful is the deliberate misinterpretation of an acronym in a comic vein, such as the World War I explanation of AEF as after

England failed, American Excavationary Forces, or ass end first and of YMCA as you must come across or your money cheerfully accepted (Lighter 1972). The comic interpretation may also be achieved by reversing the spelling of the acronym, as when RAMC for Royal Army Medical Corps becomes CMAR, interpreted as can't manage a rifle.

Also noteworthy in passing are pseudo acronyms like IOU, (bar)BQ, L-(train), and PU, which look like acronyms but are rather alphabetic rebuses that use letter names punningly in a form of syllabic writing. A term like PX (for post exchange) is a combination of genuine and pseudo acronymy, the P a shortening of post, but the X a rebus for ex. Another kind of pseudo acronym is a word that has been given a false acronymic etymology. Thus tip 'gratuity' is of uncertain origin, but is perhaps an adaptation of the sense 'touch lightly.' It has, however, been etymologized as an acronym from to insure promptness. Similarly, news, which is probably a calque on French nouvelles, has been imaginatively derived from north-east-west-south. The fact that such etymologies have been proposed in learned sources (Bergström 1906: 67) and are often advanced quite aggressively by laymen is testimony to the pervasiveness of acronymous thinking in these latter days.

The wide-spread use of acronyms has itself been named in various ways. Alphabet soup is a term attributed to the "happy warrior," Alfred E. Smith (Pyles 1952: 182), especially denoting the use of acronymic names for government agencies. Although unaccountably ignored by standard dictionaries, it has enjoyed continuous use since New Deal days. The practice has also been denominated initialese (Baum 1955: 110; Crowley and Thomas 1973: ix), word manufacture (Marchand 1960: 368), abbreviomania (Crowley and Thomas 1973: ix), and acronymania (Time 1970). But none of these terms, whether serious or playful, can match the appropriateness of German Abküfi, itself an acronym for Abkürzungsfimmel 'abbreviation craze' (Crowley and Thomas 1973: ix).

Acronyms belong to the more general class of blends. The latter are words that combine two or more lexical items, at least one of which has been shortened. They may mix unshortened and shortened items (sportscast from sports and broadcast), or shorten all items (quasar from quasi and stellar). They may be constructed of items with no overlapping sounds, like the examples above, or of items with overlapping (executary from executive secretary). In some cases shortening results from the overlapping of sounds (guesstimate from guess and estimate). Acronyms are distinguished from other blends by being invariably derived from writing and shortened to the initial part of the constituents. The terms blend and blending have had long use, the latter exemplified by the OEDS first from the writing of Henry Sweet.

Blends themselves belong to the yet larger class of abbreviations: words that have been shortened, whether in speech or writing or both.

The noun abbreviation in the sense 'shortened word' is first recorded by the OED from the writing of Jonathan Swift. There is something ironic about the fact that although Swift abhorred innovations in language, he is the first recorded user of the neologistic sense of abbreviation. "Most of the books we see now-a-days," wrote Swift, "are full of those manglings and abbreviations." That was in 1727. What savage indignation would lacerate his breast in the year of our Lord 1974?

5. The Future of the Acronym

It is natural to look for an evolutionary line of development in the history of the acronym, and Fritz Wölcken (1957) found one consisting of five major steps: abbreviations (W.Va.), alphabetisms (GOP), orthoepic acronyms (Nato) with added vowels if necessary (SNCC "snick"), apposite acronyms (Wrens), and prefabricated acronyms (Waves). Such a line of development is indeed suggested by the fashion in acronyms during the last century. It is worth remembering, however, that ichthus, an acronym nearly two millennia old, is also prefabricated. And thus, whatever the recent fashion may have been, prefabricated acronyms (illustrated by the watery Waves, ichthus, and Lacus) have been around for a long while.

Although they have an ancient heritage, acronyms of all types have increased in popularity in the twentieth century. It is natural also to wonder about their future. Acronyms thrive in the hothouse environment of literacy. In the past, their greatest use has been among communities with a high regard for the value of the written word. Thus the Kabbalists, who thought of the Torah as the first creation of the Almighty and, in its entirety, a cipher for the Ineffable Name, raised acronymy to the heights of mystical vision. Though in many ways different from Jewish mystics, the new mandarins of bureaucracy, whether civilian or military, share with the doctors of the hidden tradition a respect for the letter of the law, especially in triplicate. And thus the hierarchies of government and business have also favored the use of acronymic abbreviations.

But regard for writing is not the sole impulse to acronymy. There is also something secret, esoteric, exclusive about the acronym. Every acronym is a puzzle to be solved, a mystery to be revealed to the initiate, a password that admits the user to whatever select group has knowledge of the form. The mystic and magical traditions have always had their words of power, their barbarous names of invocation; and the bureaucrat has his--his HUDs and HEWs, his DODs and DOSes, his CINCPAC-CINCPOAs--what spirits from the vasty deep will they call forth? Knowledge is power, and knowledge of the right acronyms is requisite for those that would sit in the seats of the mighty.

Acronyms, like all other abbreviations, say multum in parvo. The busy and important man cannot take time to speak or write in full, so he must abridge his words. The more he abridges, the busier he must be; and the

busier he is, the more important we must understand him to be. As Chaucer wrote of his lawyer, "Nowher so bisy a man as he ther nas," and we may forgive the cynicism of his afterword, "And yet he semed bisier than he was." The use of acronyms is partly a way of announcing one's own importance, and so it is a popular form of self-advertising.

But there is yet another characteristic of the acronym. It is clearly an artifact, a thing that men have made and can make again; to make words is an act of poetry, and he who does so is a maker in the Renaissance sense, a sharer in the act of creation. In the making of acronyms there is a spirit of poetic playfulness. As perhaps the most easily made kind of word, the acronym gives scope for the impulse to creativity that is supposed to be a hallmark of all human language. While linguists study language, poets renew it; the acronym lets Everyman indulge the impulse to be at once linguist and poet, to produce new words playfully and to examine them as he does so. Thus acronyms have a sporty air about them that accounts for much of their appeal.

Acronyms are one way language has of paying homage to its written mode. They are works of art to be constructed and secrets to be unraveled. They communicate in the briefest possible way. They are secret passwords by which the user can identify himself as one of the ingroup. They are playthings for the poet, icons for the mystic, tools for the bureaucrat, and data for the linguist. And anything that can serve all those ends has its future assured.

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MARKEDNESS IN SERBOCROATIAN WORD ACCENT

by

Igor Miletic, University of Chicago and University of Illinois at Chicago Circle

The standard dialect of Serbocroatian, the Neoštokavian[1], has been traditionally described as having four distinct accents. Each stressed syllable has, in addition to stress, a pitch contour. This contour may be either rising or falling. Each syllable, whether stressed or not, may also be long or short.

The accents are written as follows:
1. Short falling ȕ ; indicates a falling tone on a short vowel.
2. Short rising ȍ ; indicates a rising tone on a short vowel.
3. Long falling â ; indicates a falling tone on a long vowel.
4. Long rising á ; indicates a rising tone on a long vowel.

Length in unaccented syllables is indicated by a macron ā . Unaccented brevity is indicated by a lack of diacritics.

Distribution of the accents:
1. An accent may not occur on the last syllable of a word.
2. An accent with a falling pitch (ȕ or â) may occur only on the first syllable of the word.

prȕjatelj	'friend'
pûtnīk	'traveller'

3. An accent with a rising pitch (ȍ or á) may occur on any syllable except the final.

vȍda	'water'
trȁva	'grass'
napȉsati	'to write' perfective
dubȉna	'depth'

Distribution of length

Any vowel, stressed or not, may be either long or short, except that those occurring in the pretonic position may be short only[2].

Stressed long

prâvda	'justice'
glȁva	'head'

Unstressed long

pûtnīk	'traveller'
ŭčenīk	'student'

Both length and accent are phonemic. Consider the following oppositions:

	Short falling vs. short rising			
ȕmiti	'to know'	vs.	ùmiti	'to wash'
	Long falling vs. long rising			
Lûka	'Luke' (name)	vs.	lúka	'port'
	Short falling vs. long falling			
pȁs	'dog' vs.	vs.	pâs	'waist'
	Short rising vs. long rising			
slàgati	'to tell a lie'	vs.	slágati	'to stack'

The place of accent can also be phonemic, as the following minimal pair will attest:

tèlešce	'small calf'	vs.	telèšce	'small body'

Unaccented length can be phonemic in respect to unaccented brevity.

sa sùprugom	'with the husband'	vs.	sa sùprugōm	'with the wife'[3]

On the basis of the above, it has been proposed by Roman Jakobson that the falling accents are predictable as to place of occurrence, since they can only occur on the first syllable of the word while the rising accents are not, since they can occur on any syllable except the final[4].

The Jakobsonian system of notation, therefore, would omit writing the falling accents and would confine itself to writing only length, which is not predictable, and the rising accents. A group of words using the Jakobsonian system is presented below:

Traditional marking	Jakobsonian	
brȁt	brat	'brother'
brȁta	brata	'brother' gen. sgl.
grâd	grād	'city'
grâda	grāda	'city'
nòga	noga	'leg'
rúka	rūka	'arm'

At a first glance, such a system of notation would appear to be satisfactory. However, there are other accentual phenomena in the language which pose problems for this interpretation. The underlying

assumption is that if there is not a rising accent somewhere in the word, the falling accent, either long or short, will automatically appear on the first syllable. I intend to prove that this assumption, though warranted in most instances, is not always true.

There are certain words in Serbocroatian which are unaccented and which, in certain dialects[5], attract the accent from the word immediately following. This group of unaccented words (proclitics) consists of prepositions governing the oblique cases[6]. The following are some of the proclitics:

od	'from'
u	'into'
o	'about'
oko	'about, around'
bez	'without'

It is important to note that the proclitics form a phonological unit with the following word, since it is only in this environment that they have no stress of their own. When pronounced in isolation (as for instance when given in quotation form) they of necessity acquire an accent, the short falling (\\ ). Thus, there will be ambiguity in the following[7]:

Forma ȍko je dana kao primjer.
'The form about is given as an example.'

Forma ȍko je dana kao primjer.
'The form eye is given as an example.'

The Jakobsonian system would write ȍko 'eye' and oko 'about' in the same manner, namely as oko, since the word for eye has the falling accent which is predictable and need not be written. Such a system would create hopeless confusion in the following:

oko djȁvola	'about the devil'
ȍko djȁvola	'the eye of the devil'

by writing both as oko djavola.

In the dialect which retracts the accent onto the proclitic[8], the phrase in question would show the retracted accent as short falling. The two phrases would still be distinct from each other:

ȍko djavola	'about the devil'
ȍko djȁvola	'the eye of the devil'

The Jakobsonian system would also fail to show the difference in this dialect. However, if one were to set up a system of word junctures, say a single juncture # and a double juncture ##,

and state that the rules of accent retraction operate only between double junctures and across single junctures, the Jakobsonian system could be made to work for the dialect which retracts the accent onto proclitics. The proclitic would be separated from the accented word by a single juncture #, while the words bearing independent accent would be separated from each other by double junctures ##. Then, the phrase meaning 'the eye of the devil' would be written

##oko##djavola##

while the phrase meaning 'around the devil' would be written

##oko#djavola##

and the rules of accent retraction would then correctly place the accents.

This device, however, would not work for the dialect which fails to retract the accent onto proclitics, since the word junctures cannot account for the phrase oko djȁvola 'about the devil' with unretracted accent.

This means that the Jakobsonian system, while superficially adequate for one dialect, fails to account for the other. A system which would account for both dialects would be more satisfactory. I intend to propose such a system.

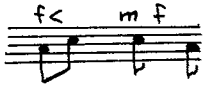
The Neoštokavian system of four accents is of comparatively recent origin. It developed in the fifteenth century, as a result of the so-called Neoštokavian accent shift, which shifted the original stress back by one syllable, whenever possible. The shifted stress then acquired, according to the standard descriptions, a rising intonation. A comparison of Neoštokavian with Older Štokavian (which has retained the original place of stress) will clearly demonstrate the nature of the Neostokavian accent shift.

Neoštokavian	Older Štokavian[9]	
pòtok	potȍk	'creek'
màknuti	maknȕti	'to move'
kràljica	kraljȉca	'queen'
pòčēti	počêti	'to begin'
dèvōjka	devôjka	'girl'
pítati	pītȁti	'to ask'
páliti	pālȉti	'to burn'
rúčak	rūčȁk	'lunch'

It can be easily seen from the above that ╲ represents an accent shifted onto a short syllable while ╱ represents an accent shifted onto a long syllable[10].

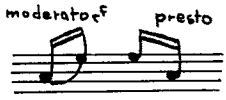
A detailed look at the rising accents will reveal that they do not merely consist of a rising tone on the stressed syllable, as has been assumed by many Serbocroatianists[11] The first scientific description of Serbocroatian accents was done by Masing[12] Briefly stated, his main claim is that the rising accents, both short and long, are bisyllabic in nature and spread over tonic (traditionally marked) and the posttonic syllables. As will be seen later, this bisyllabicity of the rising accents is crucial in determining the phonological role of accent. Masing gives the following schemes in terms of musical movement·

1. Long rising (/)



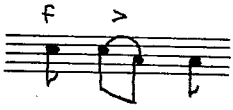
nā rŏ dĭ 'peoples'

2. Short rising (\)



žĕ nă 'woman'

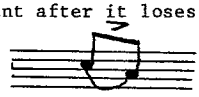
3. Long falling (⌒)[13]



să čū vam 'preserve' 1st sgl.

4. Short falling (\\)

The short falling accent, according to Masing, is the closest to simple stress. A strong expiratory stress is felt, so that the consonant after it loses clarity.



dĭm 'smoke'

Masing's findings can be summarized as follows:

1. Only the falling accents are monosyllabic. Both tone and stress are confined to the tonic syllable.

2. The rising accents are bisyllabic. The tone rises during the tonic syllable and descends during the posttonic syllable. The stress is equally distributed over both syllables.

3. The second part of the rising accent (i.e. the posttonic syllable) is similar to the falling accent.

There has been a strong reaction to Masing's findings among some Serbocroatianists. Belić[14], for example, while accepting the bisyllabicity of tone, strongly disagreed about the stress. He felt that stress is not a very prominent feature of the prosodic system and that it cannot be really measured, only felt.

The most recent research done by Lehiste and Ivić[15] with the aid of a sound spectrograph, seems to confirm Masing's analysis. As far as the short accents are concerned, there is no difference in tone between the rising and the falling accents. The information is carried entirely by the posttonic syllable. As far as the long accents are concerned, while there is evidence of respective upward and downward movement within the tonic syllable, the main information carrier is still the posttonic syllable. In the case of the rising accents, the posttonic syllable has a rather high fundamental frequency, while in the case of the falling accents it has a low one.

> While the distinction between the two long accents might thus conceivably be based on the information contained in the accented syllables themselves, in the case of the two short accents this information is decidedly insufficient. The feature which was constantly present and appeared to carry the main burden of distinction was the relationship between the stressed and the posttonic syllable.[16]

While stress does not seem to play too prominent a part, as was pointed out by Belić, Lehiste and Ivić found evidence that there are some remains of stress on the posttonic syllables following rising accents.

> The Neoštokavian accent shift has resulted mainly in changes of duration and vowel quality; the fundamental

frequency and intensity patterns that may be assumed to have been present before the shift still appear to be associated with the originally stressed syllables.[17]

Other investigators have written about the bisyllabicity of the rising accents. Carleton T. Hodge[18], for example, has devised a system of geminate vowels (to account for the long syllables) and stress marks, which will account for the differences between the rising and falling (as well as short and long) accents in terms of simple stress rather than intonation. Hodge's system can be symbolized as follows:

1. Short falling (\\ ) V́
2. Long falling (∩) V́V.
3. Short rising (\ ) V́(C)V́(V)
4. Long rising (/) VV́(C)V́(V)

Hodge's system, however, is complicated by the fact that he recognizes a category of secondary stress, which he marks as \ ,

Hodge's secondary stress	Traditional marking	
náajvìše	nâjviše	'the most' or 'the tallest'

Lehiste and Ivić, who have to date conducted the most exhaustive study of Serbocroatian accent, make no mention of secondary accents in their work.

The following two conclusions may then be drawn concerning the phonetic quality of the Neostokavian accents:

1. An accent may be either monosyllabic (as in the case of the falling accents) or bisyllabic (as in the case of the rising accents).

2. A falling accent is an invariant feature of stress. It appears on the first syllable in the case of the monosyllabic accents, or on the second syllable in the case of bisyllabic accents.

It is obvious from the above that the Neoštokavian accent shift has not, as has been heretofore assumed, simply shifted the stress onto the preceding syllable. It has rather spread the stress over two syllables and created a rising-falling or even-

falling tonal curve over them, which contrasts with the tonal curve of the unshifted stress. It is of little importance whether the stressed syllables had a falling tonal curve before the shift. The evidence from Older Štokavian indicates that they did[19] It is however just as possible that they developed or reinforced the falling tonal curve in contrast to the new rising tone which appeared on the syllables with retracted stress.

In view of the above, it becomes obvious that the falling accent is not the unmarked one at all, since it may appear on any syllable of the word. It just happens that in non-word initial position it extends a rising or even tone over the preceding syllable. This can be formalized by the following rule:

$$V(C)\overset{\backslash}{V} \rightarrow \overset{\frown}{V(C)V}$$

This means that the rising accent is the unmarked one. It appears as an invariant feature of the falling accent whenever the latter is not in the word initial position. Furthermore, a notational system based on the strictest economy would only write the falling accents and the second half (i.e. the post-tonic syllable) of the rising accents and length. In other words, the accentual system of Neoštokavian can be adequately represented by the information contained in Older Štokavian accentuation.

FOOTNOTES

[1]Neoštokavian is the dialect spoken in eastern Herzegovina and western Serbia. It has been accepted, with some changes, as the literary norm by all speakers of Serbocroatian.

[2]c.f. A. Belić, Osnovi istorije srpskohrvatskog jezika I, fonetika (Belgrade, Nolit, 1960) pp. 148-62.

[3]Sùprugom, with the short o, is the instrumental singular of sùprug 'husband', while sùprugōm, with the long ō, is the instrumental singular of sùpruga 'wife' Thus, it is only the difference in length that carries the difference in meaning.

[4]Roman Jakobson, Die Betonung und ihre Rolle in Wort-und Syntagma-phonologie, 1931, reprinted in Selected Writings I, (the Hague, Mouton, 1962) pp. 117-36.

[5]The accent is retracted onto proclitics in that dialect of Neoštokavian spoken in its area of origin, i.e. eastern Herzegovina and western Serbia. Neoštokavian spoken in other areas, particularly in the larger cities, is characterized by an absence of retraction.

[6]It could be argued that certain verbal prefixes fall into the same category. However, the rules of accent retraction in verbs are somewhat different, since the prefixed verbs which retract the accent are almost completely lexicalized so that the process of accent retraction in verbs is no longer a living one.

[7]c.f. Thomas F. Magner and Ladislav Matejka, Word Accent in Modern Serbocroatian (University Park and London, Pennsylvania State University Press, 1971) p. 20.

[8]The Neoštokavian spoken in eastern Herzegovina and western Serbia.

[9]Belić, op. cit. p. 158.

[10]A comparison with the Čakavian dialect, which has preserved an
1 extremely archaic accentuation, shows that the Older Štokavian has the original place of stress. The Čakavian place of stress (or accent) always coincides with the Older Štokavian, c.f. Čakavian potòk 'creek', maknùt 'to move', etc., even though the quality of some Čakavian accents is different from Older Štokavian; Čakavian počét, Older Štokavian počèti 'to begin'. The differences between the Štokavian and Čakavian accentuations, however, are outside the scope of this paper.

[11]Most grammars of Serbocroatian describe the accents as being either rising or falling and confined to the tonic syllable. Daničić (1925) is one notable exception. He simply states that the accents are different, but does not specify how.

[12]Leonhard Masing, Die Hauptformen des serbisch-chorwatischen Accents. (st.-Pétersbourg, 1876. Mémoires de l'Académie Imperiale des Sciences de st.-Pétersbourg, VIIe série, t. 23, no. 5.) pp. 59-71.

[13]Vuk's dictionary of 1852 gives this form as sàčūvam. However, this is a prefixed verb, composed of the prefix sa + the stem čūvam. It is possible that the speaker that Masing tested did not retract the accent.

[14]op. cit., p. 153.

[15]Ilse Lehiste and Pavlé Ivic, Accent in Serbocroatian. Michigan Slavic Materials no. 4. (Ann Arbor, University of Michigan, 1963).

[16]Ibid, p. 132

[17]Ibid, p. 134

[18]Carleton T. Hodge, Serbo-Croatian Stress and Pitch. General Linguistics, vol. III, no. 2. pp. 43-54.

[19]Belić, op. cit.

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GREEK OCCUPATIONAL TERMS WITH SEMITIC COUNTERPARTS

by Saul Levin
State University of New York at Binghamton

Glimpses of life in very remote times are the joy of historical and comparative linguistics. Often they confirm the findings of archaeology and make them more explicit. But they are just as fascinating when they bring out something of which we would otherwise have no inkling. I present linguistic evidence, chiefly Greek and Hebrew, on ways of earning a livelihood. The parallels are phonetic as well as semantic, and point to prehistoric contact.

1. The etymology of Λάϊος

In Greek it was normal for a person's name to be clearly meaningful, although it might very well be unsuited to the individual who had received it in infancy (e.g., Ἑρμογένης 'Hermes' offspring,' who grew up to be an inept business man; Plato, *Crat.* 383b-384c, 429b-c). A minority of names were opaque and presumably from a foreign language, not being used in Greek as ordinary substantives or adjectives. At the same time there were some that happened to be used only as names but were susceptible to a straightforward Greek interpretation. Such a name is the Theban Λάϊος, Oedipus' father (Aeschylus, *Septem* 745; Sophocles, *O.T.* 103, *O.C.* 220; Euripides, *Phoen.* 9, 13; Herodotus 4.149.2; Plato, *Leges* 8.836c, etc.). Since the –αϊ– does not contract to one syllable in Attic, it is reasonable to infer that a consonant **w* separated the two vowels prehistorically, as is established in many other words by a comparison of the Greek dialects.[1] So Λάϊος would be derived from λᾱός 'folk' (*Il.* 2.99, etc.). Our literary texts, including Homer, are spelled in the Ionic alphabet without F; but dialect inscriptions exhibit men's names such as ΛΑΓΟΠΤΟΛΕΜΟΣ, ΓΙΟΛΑΓΟΣ.[2] 1.1

According to Pape-Benseler, Λάϊος means 'Sohn des Volks' like *Theodo* in Old High German.[3] The sense '(son) of the people' fits the morphology of Λάϊος neatly (cf. Τελαμώνιος Αἴας 'Telamon's son Aias,' etc.). But if the implication is 'a commoner,' that is surprising when we think of his royal role and personality in the myth of Thebes, as told by the poets. For the tendency of myths is not to accept a name incongruous to the character, frequent as this may have been in real life. Since the myths were transmitted over generations, even those characters that were originally actual persons were modified in the retelling, and so was an untoward name likely to get replaced. 1.2

Another approach to the meaning of Λάϊος is through the virtual synonymy of λᾱός and δῆμος. The immediate result of applying this equation to Λάϊος is still more of a surprise. As δῆμος was the normal Attic word for 'people' and was plentifully used in prose literature, so its derivative δήμιος belonged to the ordinary language while Λάϊος remained only as a proper name. We seem, so far, justified in inferring that the prehistoric meaning of Λάϊος would have devolved upon δήμιος after a time when the two were interchangeable. It is shocking, however, to realize that δήμιος nowhere occurs in a context where we can understand it as 'son of the people'; rather, when it refers to a man, he is the public executioner or torturer (Aeschylus, *Eum.* 160; Lysias 13.56; Plato, *Rep.* 4.439e, *Leges* 9.872b-c; Aeschines 2.126; Aristotle, *Ath.* 45.1; Plutarch, *Them.* 22.2 [123a]; etc.). As an adjective it can go with anything that happens to be public: οὔ τοι δήμιός ἐστιν/ οἶκος ὅδ' 'this house is not public' (*Od.* 20.264-265); αἱσυμνῆται δὲ κριτοὶ ἐννέα πάντες/ δήμιοι 'picked umpires stood up, all nine, public [officials]' (8.257-258). But the δήμιος *par excellence,* who needs no substantive to identify his function, is the executioner. Hence the bilingual glossaries give δήμιος *carnifex* and conversely *Carnifex* δημιος· σαρκοβορος.[4] 1.3

Whereas δήμιος indicates public employment (not parentage), it is an enigma how the same can be true of Λάϊος. The solution remains hidden until we scrutinize not only the details of the Theban myth but also a non-Greek parallel. In a previous study I have explained Ἰοκάστη, Laius' wife, as 1.4

1. Eduard Schwyzer, *Griechische Grammatik,* I (Munich, 1938; Handbuch der Altertumswissenschaft, zweite Abteilung, erster Teil), 228, 241, 244, 247. The accusative Λάϊον in Pindar, *Ol.* 2.38, was corrected to Λᾷον by Hermann to obtain perfect metrical correspondence with 2.18,58,78; but 2.98 corresponds inexactly even so.

2. Both are from Corinth. Paul Kretschmer, *Die griechischen Vaseninschriften, ihrer Sprache nach untersucht* (Gütersloh, 1894; repr. Hildesheim, 1969), pp. 24, 44, 46.

3. *Wörterbuch der griechischen Eigennamen,* 3d ed. (Brunswick, 1863-70; repr. Graz, 1959), II, 762.

4. G. Goetz (ed.), *Corpus glossariorum Latinorum,* II (Leipzig, 1888), 97, 269.

a Greek adaptation of a Semitic name, יוֹכֶבֶד {yōwkébed} in Exodus 6:20 and Numbers 26:59.[5] Besides the phonetic similarity, the motifs common to Oedipus' mother and her Hebrew counterpart are overwhelming. These include incestuous marriage to a younger man, with paradoxical issue – out of four children two famous though jealous sons and one famous daughter, but on the other hand something hideously deformed; in addition to all that, the box, תֵּבָה {tebā́h} in Hebrew from Egyptian, in which Jochebed's baby was set afloat and Jocasta's too according to one version – so that the hero was Θηβαῖος in a double sense, as a foundling rescued from a drifting box and as a resident of Thebes in Egypt or in Greece, whether by adoption or by birth. The data in the Bible are less blatant than in Greek poetry, but no more ambiguous. Furthermore, Jochebed was the daughter of לֵוִי {lewī́y}, which sounds suspiciously like Λάϊος – Λάϊε in its simplest form (i.e., the vocative).[6]

Jocasta's husband, Jochebed's father – the discrepancy matters less in a family notorious for incest. But we need not shrug it off with a bit of cynical moralizing. For the discrepancy is re- 1.5
solved when we read the Septuagint of Numbers 26:59 (with the end of 58): καὶ ΚΑΑΘ ἐγέννησεν τὸν ΑΜΒΡΑΜ. καὶ τὸ ὄνομα τῆς γυναικὸς ΑΜΒΡΑΜ[7] ΙΩΧΑΒΕΔ θυγάτηρ ΛΕΥΙ ἣ ἔτεκεν τούτους τῷ ΛΕΥΙ ἐν Αἰγύπτῳ· καὶ ἔτεκεν τῷ ΑΜΒΡΑΜ τὸν ΑΑΡΩΝ καὶ Μωυσῆν καὶ ΜΑΡΙΑΜ τὴν ἀδελφὴν αὐτῶν 'and Kaath begat Amram. And the name of Amram's wife was Jochebed, daughter of Levi, who bore these [the aforesaid in 26:57-58] to Levi in Egypt; and she bore to Amram Aaron and Moses and Miriam their sister.'[8] Amram was both grandson and nephew to the woman he married! Apart from the succinct statements of the Bible are some Talmudic elaborations, which without acknowledging any scandal corroborate it to this extent: they say she was married for the second time and very old when Moses was born.[9]

That neglected Greek text, near the end of a long chapter on the genealogy of the tribes of Israel, not only opens our eyes to Levi and Jochebed and their progeny.[10] It also sets Laius and 1.6
Jocasta in a new light. The Greek poets concentrated so on the incest unwittingly perpetrated by Oedipus that the antecedents of it became ancillary – how he was *misbegotten* in the first place. Homer indeed in his few lines (*Od.* 11.271-280) omits the background, leaves Oedipus' father unnamed, and calls the mother Ἐπικάστη; his myth of Oedipus has least to do with the Hebrew. But the other poets, who make Oedipus the son of Laius and Jocasta, are bound by their sense of myth to present the deeds of Oedipus when full-grown as somehow in retribution for what his father (or his parents) did. Yet the Greek narrative genius, as it developed the theme of the oracle warning Laius,[11] used this to motivate what was inherently improbable: that a man or his wife would have something to dread in the birth of a child – the normal outcome, the very fruit of marriage. Unless their union were somehow sinful, why should the offspring be misbegotten?

5. "Jocasta and Moses' Mother Jochebed," in Proceedings of the Second International Conference on Boiotian Antiquities (at McGill University, Montreal), forthcoming as Supplement 2 to *Teiresias.*

The Hebrew compound name means, approximately, 'The-LORD-is-Glorious.' It is consistent with the woman's sorrows, provided that we understand the general significance of theophoric names: *touched* by God, not necessarily or permanently *favored;* cf. Ἡρακλῆς. The awesomeness of divinity becomes manifest through spectacular misery and horror, no less than through spectacular deliverance.

6. Found in the oracle quoted in the ὑπόθεσις accompanying Sophocles, *O.T.*, and Euripides, *Phoen.*

7. ΑΥΤΟΥ 'his' instead of the name in codex B – a semantically unimportant variant. Elsewhere in the passage, B has the spelling ΑΜΡΑΜ (also ΛΕΥΕΙ instead of ΛΕΥΙ).

8. The extant Hebrew text, both the Massoretic and the Samaritan, differs by just one letter. It has אתה where the *Vorlage* of τούτους must have been אלה; that is enough to give a different though feeble sense: 'Jochebed, daughter of Levi, whom she [unspecified; presumably Jochebed's mother] bore to Levi in Egypt.' I regard אתה in place of אלה as one more instance of a תקון סופרים {tiqúwn ṣōwprī́ym} 'correction of the scribes,' of which eighteen examples are noted in the rabbinical and Massoretic sources; see D. Barthélemy, "Les tiqquné sopherim et la critique textuelle de l'Ancien Testament," *Vetus Testamentum,* suppl. vol. IX (1963), 285-304. One of the eighteen, in Num. 12:12, also gets rid of a blot on the reputation of Aaron's and Moses' mother.

9. Sotah 12a, Baba Bathra 119b-120a. The rabbis' account that Amram was both her first and her second husband is clumsy in itself, but made more artistic by linking the divorce in between to Pharaoh's decree that every male child of the Hebrews should be killed (Ex. 1:22).

10. My surname reminds me I am talking about ancestors of mine, however remote. I will not pretend it makes no difference. But neither would I stretch the commandment "Honor thy father and thy mother" so as to reach back endlessly into the past. It is one thing for a writer to publish, from his parents' own letters and diaries that fell to him, the ugly facts of their sexual disorders – facts which they kept private during their lifetime and never explicitly authorized anyone to divulge afterwards; Nigel Nicolson would have done better to forgo a literary success built upon such material. But it is something else to rummage, as I have done, in a very old attic that was never locked, and then to make known what was really in the public domain all along. Furthermore, as this study proceeds, the ancient house of Levi will be viewed not just more clearly but more sympathetically than was possible beforehand. First it is necessary to state the sad truth about this odd family; otherwise they would not be so fascinating.

11. See my article, "'Know Thyself': Inner Compulsions Uncovered by Oracles," in *Fons perennis: Saggi critici di Filologia Classica raccolti in onore del Prof. Vittorio D'Agostino* (Turin, 1971), pp. 236-249.

In real life, from time to time, lawful wedlock must have produced a deformed baby; but the imagination, the human faculty which looks at things mythically, would surmise that the poor infant was misbegotten and reflected a parental sin. The sad parents, in their disappointment, would be most prone to blame themselves, or each other. The risk was doubtless greater when they were blood-relatives; whatever bad genes were in one of them, even though recessive, half of those would also be in the other one and would combine to make a defective child. An added factor might enter when the parents were of two separate generations, the mother being either too old or unduly young. Given such perils of incest, any abandoned and unsightly infant – even if salvaged by kind-hearted foster parents – could arouse in the minds of unsympathetic or gossipy neighbors the foul suspicion of *tainted* birth.[12] 1.7

When the Greek poets other than Homer attached the names **Λάϊος** and **Ἰοκάστη** to the parents in the Oedipus myth,[13] they did so without "upstaging" the hero himself. Had they narrated the marriage of Laius and Jocasta along the lines of Levi and Jochebed in the Septuagint, the sequel of Oedipus killing his father and marrying his mother would have been anticlimactic. Consider Aeschylus' masterpiece, the *Orestea.* To judge from the opinions of critics and the unequal popularity of the three component plays, the high point comes in the first of them, when Agamemnon is murdered by his alienated and deceitful wife. Orestes' revenge in the second play of the trilogy cannot match the brilliance and horror of the earlier murder, although Aeschylus presumably meant him to be the central personage. From another trilogy of Aeschylus we have only *Seven against Thebes,* the third play; the fragments of his *Laius* and his *Oedipus* are too meager to prove anything.[14] But I venture to argue, from the silence of all extant scholia and mythographies, that Aeschylus and the other authors whom they drew upon had said nothing to make Oedipus' incest a *retaliatory* judgement of God on his parents because of their own prior incest. 1.8

Yet (as I argued a while ago), without incest on their part, the motive for abandoning their first-born infant son is far-fetched. The name **Οἰδίπους** bespeaks a deformity of the feet, which could have been the cause of their determination to get rid of him – rather than the consequence of it, as in Sophocles' narrative (*O.T.* 717-719, 1031-1036).[15] But such a birth-defect, in the logic of myth, is itself evidence of the parents' guilt. So we come back to the enigma of Laius' and Jocasta's marriage; and this, along with the enigma of **Λάϊος** = **δήμιος** = *carnifex,* is clarified by the Hebrew {lewíy}. 1.9

λα(ϝ)ός, unlike **δῆμος,** has no good Indo-European etymology.[16] {lewíy}, on the Hebrew side, appears to contain the suffix {–íy} of gentilic derivatives, like **–ιε, –ιος** in Greek.[17] However, no unsuffixed noun, *{lew} or whatever, is found in Hebrew or any Semitic language, so as to constitute a base for the formation of {lewíy}.[18] It therefore remains a mystery which language was the source of these terms known from Greek and Hebrew; and that ultimate etymology may be forever elusive, since many ancient languages of the eastern Mediterranean have been lost. But we have enough left in the well documented ones to explain how {lewíy} indeed designates the same status or occupation as **δήμιος,** and hence to infer the same about **Λά(ϝ)ιος** originally, because it is the indispensable middle term, phonetically like {lewíy} but semantically parallel to **δήμιος** as **λα(ϝ)ός** to **δῆμος.** 1.10

One narrative in the book of Genesis (34:25 ff.) and many genealogies present {lewíy} as a proper name, on nearly the same footing with those of Jacob's other sons. But elsewhere {lewíy} is 'a Levite,' a specific sort of itinerant, hereditary occupation (e.g., Judges 17:9). The definite article 1.11

12. The author of Ex. 2:1-3 is at pains to point out that the child who was later named Moses by his Egyptian foster mother had nothing wrong with him at birth and was not immediately exposed by his mother, the daughter of Levi, but kept hidden as long as possible. Nevertheless, when grown up he complained of being "uncircumcised of lips" so that Pharaoh would not listen to him (6:12,30). It sounds like a visible disfigurement along with a speech impediment.

13. Prompted not only by similar subject-matter but also perhaps by assonance between the Homeric Ἐπικάστη and the Ἰοκάστη or *Ἴοκάπτη of Semitic origin.

14. Carl Robert, *Oidipus: Geschichte eines poetischen Stoffs im griechischen Altertum* (Berlin, 1915), I, 252-257.

15. Robert, I, 62-63; II, 27-28.

16. Hjalmar Frisk, *Griechisches etymologisches Wörterbuch* (Heidelberg, 1960-70), I, 380-381; II, 83-84.

17. Here I am indebted to John Pairman Brown's unpublished research.

18. Old South Arabian had לוא {lwʔ} (vowels unknown) and the feminine לואת {lwʔt}, which apparently meant 'priest(ess)' or something of the sort; Hölscher in Pauly-Wissowa, *Real-Encyclopädie der classischen Altertumswissenschaft,* XII, 2156, with bibliography.

לוי is etymologized by his mother Leah with a verb of the reflexive conjugation יִלָּוֶה {yil·owéh} 'he will adhere, accompany, be attached' (Gen. 29:34; cf. Num. 18:2,4). The wives of Jacob do this in turn with each of their sons and their maids' sons. This verb in other conjugations, or possibly a homophone of it, means 'borrow' (e.g., Deut. 28:12). So the original sense of {lewíy} may be 'under obligation.'

can be prefixed to it: הַלֵּוִי {hal·ewí^y} 'the Levite' (Ex. 4:14, etc.) – which is not done to a normal proper noun in Hebrew. It can also be pluralized, הַלְוִיִּם {halwiy·ím} 'the Levites' (Ex. 38:21, etc.); and often it is used as an adjective, following the noun in typical Hebrew fashion: הַנַּעַר הַלֵּוִי {han·áɜar hal·ewí^y} 'the Levite boy' (Judges 18:3,15), הַכֹּהֲנִים הַלְוִיִּם {hak·ohăní^ym halwiy·ím} 'the Levite priests' (Deut. 17:9; frequent in this book and in Joshua, Ezekiel, Ezra-Nehemiah, and Chronicles).

While the {lewí^y} in Genesis is made out to be the progenitor of every {lewí^y} after him, the ending is the same as though he too were classed as one of them. Thus the morpheme {–í^y} in {lewí^y} does not signify 'son of so-and-so' as it does in many other Hebrew terms. Rather, like the Greek suffix in δήμιος, it stands for another sort of relationship, which obtained (as I will show) in all the texts from Genesis onward. The first {lewí^y} was already 'a Levite' – i.e., what would have been called δήμιος in classical Greek or *carnifex* in Latin. 1.12

The essense of his social position was that he lacked independent means – livestock or farm land – and had to be supported by the people. In return he performed for them certain work – distasteful, unprofitable, bloody – that had to be done. It was quite different from the productive labor for which a man would buy slaves or take on hirelings if it became too much for him and his sons. The need for a {lewí^y} was occasional; rare was the individual that could give him steady employment (Judges 17:7 ff.); but by making himself available to the people at large he gained a small livelihood. He was well enough adapted to the economy, such as it was, so that his trade and his name did not disappear. 1.13

The very first deed of {lewí^y} to be mentioned in the Bible is one of public vengeance on behalf of the sons of Jacob for the seizure and rape of their sister Dinah (Gen. 34:25-26). It was no war; the men that he and Simeon killed with the sword were helpless from their recent circumcision, which had united them with the girl's kinsmen (34:15-16,22). After the two had finished killing, the sons of Jacob as a whole plundered and carried off everyone and everything that had belonged to the dead men;[19] this confirms that Simeon and Levi served as executioners. Old Jacob's last words about them were: 1.14

> "Simeon and Levi [are] brothers; their knives[20] tools of violence.
> Let not my breath come into their company, nor let my honor be united with their caucus;
> for they have killed a man in their anger and hamstrung an ox at their pleasure.
> Cursed [is] their anger, for it is powerful, and their outburst, for it is stubborn;
> I will split them up in Jacob and scatter them through Israel" (Gen. 49:5-7).

Here are so many attributes of the δήμιος or *carnifex:* sharp instruments for killing or maiming man and beast, which he does heartily; no territory of his own and no organized, self-sustaining clan; a neighbor and a pariah at the same time, shunned by the people.[21]

The next episode in which the Levites distinguished themselves was when Moses told them to take their swords and kill indiscriminately the people who had been worshipping the golden calf. This the Levites did with gusto, even to their own relatives and friends (Ex. 32:26-29). They acted on the express orders of the ruler; they had no other motive. And no resistance, or risk to themselves, is mentioned; presumably the ones they killed were drunk or exhausted by revelry. So it was an execution, not a battle or riot. 1.15

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19. See E. A. Speiser (tr.), *The Anchor Bible: Genesis* (Garden City [1964]), pp. 264-268. The Bible neither emphasizes nor palliates the treachery of the attack upon men now participating in the covenant. Jacob rebukes the two killers for putting him in danger of reprisal; they retort, "Should he treat our sister like a harlot?" (34:30-31). The Israelites demonstrated that nobody should dare to seize one of their women, not even to be his wife; for they would never recognise such a marriage.

It may be that in accord with a divergent tradition (of which we have few remnants) the executioners, being younger sons, were uncircumcised or disinherited, or both. If so, they personally were outside the covenant and did not violate it in killing the newly circumcised abductors of Dinah. One sure datum is that Moses, whose parents were both Levites, did not circumcise his son (Ex. 4:24-26). And the book of Joshua declares that those born after the exodus from Egypt – i.e., during Moses' leadership – were uncircumcised until he was succeeded by Joshua the Ephraimite (5:5). On this point the LXX and the Massoretic text agree, though they differ upon some other details in the chapter.

20. מְכֵרֹתֵיהֶם {mkeroṯeyhém}, a *hápax legómenon* from which we posit a singular form *{mkerōʰ}, close to the Greek μάχαιρα *(Il.* 3.271, etc.); Heinrich Lewy, *Die semitischen Fremdwörter im Griechischen* (Berlin, 1895), p. 177.

21. Simeon is only of incidental concern to us here. The name שִׁמְעוֹן {šimɜóʷn}, from the verb-root that means 'hear, listen,' evidently signifies 'client'; cf. *cliēns, cluēns* and the verb *clueō* in A. Ernout and A. Meillet, *Dictionnaire étymologique de la langue latine,* 3d ed. (Paris, 1951). And indeed the Simeonites were clients of the Judahites, according to Joshua 19:1,9 and Judges 1:3,17. Moses' blessing upon each of the sons of Israel (Deut. 33) conspicuously omits Simeon.

After that the Levites no longer function in the Bible as executioners, but either as priests or else as subordinate caretakers of the sanctuary. In many of the strictly Mosaic passages, such as Leviticus 1:5, it is unclear from the Hebrew text, the ancient versions, and the exegetical traditions who is meant to do the actual slaughtering of the sacrificial beasts. A major concern of the Pentateuch is to regulate, and thus to ameliorate, the status of the Levites, while remaining consistent with tradition and with practicality. The points with the most bearing upon **δήμιος** and **Λᾶιος** are the following: 1.16

(1) Numbers 16:8-10 is an explicit polemic against the claim of the Levites to the priesthood (cf. Joshua 18:7). At the very least it proves that they made such a claim at some time and that it was in dispute.
(2) In the age of the Judges, which was remembered as anarchic (17:6), a young Levite in search of a livelihood was hired (18:4) by Micah the Ephraimite to serve as priest to the cult for which Micah's mother had commissioned an image and Micah had made the other accoutrements.
(3) The Levites "get no estate among the sons of Israel" but instead are to receive tithes from them (Num. 18:21-24; cf. Deut. 10:9, 14:28-29, 18:1-2, Joshua 13:14,33). They are named among the indigent – the alien, the orphan, the widow – to be fed charitably (Deut. 26:12-13), and they are not reckoned in the census of Israel (Num. 1:48, 26:51,62, I Chr. 21:6).
(4) Especially when an Israelite comes with his family to sacrifice, he is to include the Levite among the dependents sharing in the feast (Deut. 12:11-12,17-19).
(5) The Levites camp by the sanctuary, away from any tribe of Israel (Num. 1:50-53); and separate towns are assigned to them later, so as not to live close to the Israelite settlements (Num. 35:2-8, Joshua 14:4, 21:3-41[39]). Some of the Levitical towns serve also as refuges for involuntary homicides.

The **δήμιος** was similarly stationed outside the walls of Athens (**ἔξω πόλεως κατοικῶν**, Pollux 9.10) and not allowed to live among the people.[22] Aside from putting criminals to death, and torturing, and flogging – daily work in a large state – it is doubtful whether he was ever employed at anything not quite so loathsome. Butchery perhaps; but if that was once part of his work, it was taken over in the historical period by the professional **μάγειρος** (Plato, *Euthyd.* 301c-d; Herodotus 6.60, etc.).[23] The Latin term *carnifex,* which is otherwise equivalent to **δήμιος**, ought from its etymology to mean 'butcher' (slicer, meat-maker); however, in all extant passages that is expressed by a different word *lanius* (Plautus, *Ps.* 197, etc.).[24] 1.17

When the Hebrew {lewī́y} no longer had the atrocious task of putting men to death, his work was something like what the Greeks called **λῃτουργία**, a public service at the expense of an individual.[25] The first part of this compound noun is derived from **ληός**, the Ionic form of **λᾱός** (Herodotus 5.42.2; Hipponax, fr. 88 Bergk; **λεώς** in Attic by "quantitative metathesis"). Hesychius also has a gloss **λῄοι· ἱεροί. καὶ ἄγγελοι** 'holy ones; and messengers.' This neatly matches the status of the Levites in Num. 3:12-13, 8:14-19; they are dedicated to the LORD instead of all the first-born of Israel, whom he has consecrated for himself, and as deputies of the Israelites they serve under Aaron and his sons. 1.18

The gloss of Hesychius is also precious on the phonetic side for giving the Ionic counterpart to the Boeotian **Λάιος**. Before it lost the consonant [–w–], it would have been ***ληϝι–** with virtually the same sounds as the Hebrew {lewī́y}. Only the accent clashes; for it is recessive in Greek nouns or adjectives in **–ιος** (with few exceptions).[26] 1.19

We have still to show how the meaning of {lewī́y} applies to the mythical figure of Laius. Moses' blessing upon Levi – i.e., the tribe of Levi – begins with God's gift of lots for divination, תֻּמֶּיךָ וְאוּרִיךָ {tum·mey·ḵo wə·ʾu·rey·ḵo} (Deut. 33:8). Likewise Laius was a diviner; in the sixth centu- 1.20

22. Thalheim in Pauly-Wissowa (above, note 18), IV, 2854 f. That he was a slave, as stated in the modern reference books, is an unwarranted and unnecessary inference from the adjective formation, as though a noun such as δοῦλος had to be understood with ὁ δήμιος 'the public [*sc.* slave].' Nowhere do the ancients call him that; and in the absence of such direct evidence, we should rather ask what the classification would mean: That the state had bought or captured him? That he was liable to be sold? That if he were ever murdered, someone other than his kinsmen might prosecute the murderer? The meager texts that remain to us throw no light on these questions.

23. The joke of Phoenicides (fr. 4.11-15 Edmonds = Stobaeus 3.6.13 Hense), about a physician described as πτωχὸς . . . καὶ δήμιος, is neatest if the latter term is taken in its ordinary sense of 'executioner.'

24. Donatus on Terence, *Hec.* 441, remarks, "carnifices dicti quod carnes ex homine faciant." At least there was a jocular resemblance to the butcher's trade.

25. Antiphon 5.77, Andocides 4.42, etc. The mss. have the "silver age" spelling λειτ-, but early inscriptions in the Ionic alphabet have ΛΗΙΤ- (e.g., *IG* 2.172.4 = *IG*^{2} 2-3.417.4); see K. Meisterhans, *Grammatik der attischen Inschriften,* 3d ed. revised by Eduard Schwyzer (Berlin, 1900), pp. 36-37.

26. C. D. Buck and Walter Petersen, *A Reverse Index of Greek Nouns and Adjectives* (Chicago [1939]), pp. 43-44. The comparable Hebrew formation with accented {–ī́y} is confirmed by the LXX: ΛΕΥΪ according to codex F, the only early ms. whose accents were written by the first hand; *Monumenta sacra et profana ex codicibus praesertim Bibliothecae Ambrosianae,* Vol. III, *Pentateuchi et Josue quae ex prima scriptura supersunt in codice Ambrosiano Graeco seculi fere V,* ed. A.M. Ceriani (Milan, 1865), pp. xvi, 91 (Ex. 32:28), 191 (Num. 1:49) et passim.

ry B.C. a man named Antichares advised the Spartan Dorieus "from the oracles of Laius (ἐκ τῶν Λαΐου χρησμῶν) to settle Heraclea in Sicily" (Herodotus 5.43).

Divination is Levi's reward for loyalty to God when put to the test (Deut. 33:9): "He who said to his father and mother, 'I have not seen him' [alluding to a version of the abduction of Joseph different from Gen. 37:29-32?] and did not recognise his brothers and did not know his sons [cf. the killings in Ex. 32:29; above, §1.15]." The last word, if we stick to the letters בנו regardless of the vocalization, could mean 'his son.' This recalls a crucial episode in the myth of Oedipus, when Laius did not recognise the young stranger as his son (Sophocles, *O.T.* 802-809). The correspondence is inexact; but it is mythically appropriate that incestuous families are also prone to parricide – in the true Latin sense of *par(r)icidium,* not limited to killing of the father as has been wrongly supposed through a false etymology.[27] As they ignore the most fundamental restraint upon lust, so they kill the ones who ought to be dearest to them. 1.21

The sins of Laius and his family become less grotesque when we consider their isolated social position, not as the royal house of Thebes but as marginal persons like the δήμιος and the Hebrew {lewī́y}. Destined to make do without a normal livelihood, looked at askance by the community which they depended on and which employed them for truly odd jobs, they survived and kept on their feet through a matter-of-fact crassness horrifying to others. They could not afford to be squeamish when it came to shedding blood. If they went in for incest, that is no wonder in a breed without property, subsisting on handouts, and soiled by filthy, gruesome, repulsive chores; who else would marry them with such disabilities.[28] So their plight was self-perpetuating. 1.22

If they were cold-blooded in abandoning their babies, that too was of a piece with their hard lot. Their living was exceptionally precarious and not humanly predictable; for they did not resort to robbing or stealing, nor even to outright begging. So they had need for divination to decide whether or not to raise a child, whereas more fortunate couples would have no misgivings about it. 1.23

Something of that harsh way of life could well persist in better circumstances. The Λᾶιος who was ruler of Thebes was still – I suggest – like a hereditary {lewī́y} in being apprehensive about his own offspring.[29] The name may have got attached, in the first place, to Oedipus' father (see above, §1.8) because of the very incongruity: a king behaving so undynastically, as if he were a pariah and dared not raise his own son. 1.24

We should view the foul things ascribed to the house of Laius or Levi as mostly gossip rather than fact.[30] Likely enough there was a grain of truth; myth needs no more than that for the starting point. Then people will receive the accretions as equally believable. For it ceases to matter whether so-and-so really did such-and-such. Horrors fascinate; they are memorable and worth repeating; and good story-tellers are not content to treat them in the abstract. They must connect them with someone specific. Nor do they pick a name at random. The meaning or associations that it already has determine their choice of it. That is why the varied background of Λᾶιος or {lewī́y} deserves such detailed exploration as I have subjected it to. 1.25

In both languages or cultures he was largely a focus of revulsion. Yet we note that the name did not exclude him from being king of Thebes. And Oedipus, in spite of his taint, also became king by saving the city from the Sphinx. Moses too, a Levite cast off in infancy and a homicide on behalf of a wronged Israelite (Ex. 2:11-12), returned to be the liberator of the Israelites from Egypt and their lawgiver. 1.26

In one striking way the morphological sense of {lewī́y} : Λᾶιος 'belonging to the folk' – rather than to some particular family – was upheld in practice. If he was wronged, a duty of revenge devolved upon the whole community; for his own kin were powerless, or were presumed so. Hence, when the concubine of a journeying Levite was raped to death in a Benjaminite town, he invoked and obtained a muster of all the other tribes of Israel to punish the outrage (Judges 19-20). Similarly the murder 1.27

27. Ernout-Meillet (above, note 21), p. 855; A. Walde, *Lateinisches etymologisches Wörterbuch,* 4th ed. revised by J. B. Hofmann (Heidelberg, 1965), II, 253.

28. The Mosaic law does not forbid the marriage of a Levite to an Israelite. Neither is it explicitly authorized. For the most part the Bible presents the Levites in a somewhat improved state. But the early genealogies, as well as their poverty, leave room for suspicion that the Israelites kept aloof from marrying them.

29. According to the LXX of Ex. 2:2-3, but contrary to the extant Hebrew text, three of the verb forms are plural and have the Levite father as well as the Levite mother for subject: "And she conceived in her belly and bore a male; seeing (ἰδόντες) it [to be] nice, they hid (ἐκέπασαν) it for three months. When they could (ἠδύνασθε) no longer hide it, its mother got it a box . . ." (see above, note 12).

30. In *Oedipus at Colonus* 220, no sooner does the poor blind man begin to identify himself obliquely – Λαΐου ἴστε τιν' ἀπόγονον 'you know some offspring of Laius?' – than the chorus of villagers reacts with horror: [ὦ ὦ] ἰού. Infamy attached to the *family of Laius,* even without explicit mention of Oedipus.

of Laius on the road from Thebes had to be avenged by the Thebans as a whole; and as they neglected to track down the unknown murderer, they brought upon themselves a great plague (Sophocles, *O.T.* 100 ff., especially 126-127). So the {lewíʸ} or **Λάϊος** was neither a wretch of no consequence who could be mistreated in any way with impunity, nor on the other hand a man who had behind him a family able to vindicate his rights without needing to appeal to everyone else.[31] The term is therefore of great significance in pinpointing a characteristic of society at an early stage, mostly prehistoric. It manifests a social obligation, if not an out-and-out moral conscience.

2. Compounds describing men's work

2.1 In a section of *The Indo-European and Semitic Languages* (pp. 149, 159-170, 203-204) I investigated the parallel formation of words like **αἰπόλοι** 'goatherds,' **δορυφόροι** 'spear-bearers' in Greek (*Il.* 2.474; Herodotus 1.59.5; etc.) and phrases like רֹעֵי צֹאן {róɘeʸ cóʔn} 'shepherds, goatherds' in Hebrew (Gen. 46:32). I need not repeat my findings here, beyond the most impressive point that the part indicating the doer, if formed from a biconsonantal root, has the structure [CóC°/ey] in the plural. The accent is conditionally retracted from the last syllable; for if the consonantal root is of a different sort – as in **ὑφορβοί** 'hog-raisers' (*Od.* 14.410), אֹיְבֵי אִישׁ {ʔoybéʸ ʔíʸš} 'man-haters, a man's enemies' (Micah 7:6) – the [o] that is now checked by the consonant following it in the same syllable repels the accent: [CoC-Có/éy].[32] The accentual alternation is well integrated in the phonology of Hebrew; but in Greek it seems inexplicable except through bilingual influence.

2.2 The doer in the Greek compounds comes after the object – the opposite of the Hebrew order. It cannot be straightforward borrowing; for the morphologically similar features show up in a context determined by the different rules of the two languages. Neither do the verb-roots which embody the [CóC°/ey] look cognate themselves.[33] Nevertheless these Greek and Hebrew expressions are worth studying together, because in any case they show on a large scale a perception of the variety of men's occupations. According to their form they just mean 'doing so-and-so to a certain object'; but nearly always it is an habitual activity:

שֹׁתֵי יָיִן {šóte^y yóyin} 'wine-drinkers' (Joel 1:5, cf. Pr. 23:20);
יֹשְׁבֵי שָׁעַר {yóšbe^y šóɘar} 'gate-sitters, gossips' (Ps. 69:13);

31. The dramatic irony of Oedipus is compounded because the Λάϊος whom he as present king of Thebes undertakes to avenge, for the sake of the people, turns out to have been his own father (as the audience and the prophet Tiresias already knew). As son of the murdered man, Oedipus would have been just the right one to pursue vengeance in a normal situation – i.e., had the father not been Λάϊος but a decent, innocent man.

32. In Hebrew a group of two consonants after [o] goes with the next syllable, unless the former one is [y]. Hence דֹרְכֵי קֶשֶׁת {dórke^y qéšet} 'bow-benders, archers' (I Chr. 8:40, etc.; Lydian mercenaries in Jer. 46:9; also מֹשְׁכֵי קֶשֶׁת {móške^y qéšet} in Is. 66:19 and נֹשְׁקֵי קֶשֶׁת {nóšqe^y qéšet} in I Chr. 12:2, II Chr. 17:17 = τοξοφόροι, Pindar, *Pyth.* 5.41; Herodotus 1.103.1). But the retraction of the accent to the [o] in Hebrew depends upon the next word (the object or complement) being accented on its first syllable; otherwise נֹשְׂאֵי מָגֵן {noʔsʔé^y mogén} 'shield-bearers' (II Chr. 14:7 = σακεσφόροι, Euripides, *Phoen.* 139, etc.), עֹשֵׂי מִלְחָמָה {ɘóse^y milxomóʰ} 'war-makers, warriors' (II Kings 24:16 = πολεμοποιοί, attested in the accusative πολεμοποιούς, Julian, *Ath.* 281b). However, the latter type of accentuation is infrequent in Hebrew; for the nouns chosen as object in phrases designating occupation are normally short and therefore collocate the accent of the preceding participle on the [o] rather than the [e].

33. עֹשֵׂי שָׂכָר {ɘóse^y sáker} 'wage-earners' (Is. 19:10 = μισθοφόροι, Thucydides 7.57.3,11, etc.) contains the root {ɘ–s} that means 'do, make,' not found in any other Semitic language and likely to be cognate to the Greek ἀγ–; *The Indo-European and Semitic Languages* (Albany, 1971), pp. 386-389. However, because in Greek the root begins with a vowel instead of a guttural consonant, compounds that contain it end in –ηγοί; e.g., στρατηγοί 'army-leaders, generals' (Herodotus 7.83.1; etc.).

בֹּנֵה עִיר {bóne^ʰ ɘíʸr} 'a town-builder' (Gen. 4:17) and בֹּנֵה חַיִץ {bóne^ʰ xáyic} 'building a wall; wall-builder' (Ezek. 13:19) may be compared to οἰκοδόμος 'a house-builder' (Plato, *Rep.* 2.369d,370d; οἰκοδόμοι, 2.370e; etc.), τειχοδόμος 'a wall-builder' (Pollux 1.161; etc.). The root {B–n} is phonetically somewhat like {d–m}, but the correspondence is loose. It gains a little from the recurrence of [B : d] in שֶׁבֶת {šébet} : ἕδος : Sanskrit सद्: {sádaḥ} '**seat.**'

The triconsonantal root κλ–π– 'steal,' as in ἀνδραποδοκλόπος 'a slave-stealer, kidnapper' (Sophocles, fr. 1011 Pearson = Photius, *Lexicon* 126.1 Reitzenstein) and κυνοκλόπον 'a dog-stealer' (Aristophanes, *Ranae* 605), not only is widespread among the Indo-European languages but also has a likely Semitic cognate; see my article, "'Something Stolen'" (below, §3.1). 'Kidnaper' in Hebrew is גֹנֵב נֶפֶשׁ {gonéb népeš}, literally 'soul-stealing' (Deut. 24:7), or וְגֹנֵב אִישׁ {wgonéb ʔíʸš} 'and a man-stealer' (Ex. 21:16).

עֹבְדֵי פֶסֶל {ʔobbdeʸ pésel} 'idolaters' (Ps. 97:7);
לְמַצְאֵי דָעַת {lmóc?eʸ dóʔaṯ} 'to finders of knowledge' (Pr. 8:9);
μιτροφόροι 'mitre-wearers' (Herodotus 7.62.2);
ξεινοδόκοι 'receiving guests; hosts' (*Od.* 8.543, etc.).
Or even if the context shows that the action happens to be done only once, the one time is enough to give permanent fame or status; e.g.,
ἀνδροφόνοι 'man-killers, murderers' (Plato, *Phaedo* 114a, etc.);[34]
τυραννοφόνοι 'tyrannicides' (Dio Cassius 44.35.1) = **τυραννοκτόνοι** (Diodorus 16.14.1, etc.);
τεκνογόνοι 'child-bearing' (Aeschylus, *Septem* 928, of course in reference to women).
Some, such as שֹׁלְפֵי חָרֶב {šólpeʸ xóreb} 'drawing a sword' (Judges 20:25), state a socially important function, though exercised only on occasion and not the means of gaining a livelihood, at least in this passage.

But by and large the [–o–°/eʸ] terms designate a regular and distinctive occupation, by which certain men come to the attention of the community. Dozens or hundreds of different instances can be cited either in Greek or in Hebrew. The following is only a selection, loosely grouped under six headings (and not repeating the many items that I mention elsewhere): 2.3

(a) Food-producers 2.4

γεωπόνοι 'land-workers, farmers' (Philo, *Quod deterius potiori insidiari soleat* 28[104] = 1.282.5 Cohn; עֹבֵד אֲדָמָה {ʔobéd ʔăḏomóʰ}, singular, Gen. 4:2, Zech. 13:5);
γεωμόροι 'land-owners' (Plato, *Leges* 5.737e, etc.); cf. וְשֹׁכְנֵי אֶרֶץ {wšóknеʸ ʔórec} 'and land-(country-)dwellers' (Is. 18:3);[35]
χηνοβοσκοὶ βουκόλοι 'goose-grazing cowherds' (Cratinus, fr. 46 Edmonds [*Dionysalexander*] = Athenaeus 9.384b); **οἵ τε ὀρνιθοτρόφοι καὶ οἱ χηνοβοσκοὶ** 'the poultry-raisers and the goose-farmers' (Diodorus 1.74.4);
κτηνοτρόφοι 'livestock-raisers' (Gen. 46:32,34; Philo, *De agricultura* 57 = 2.107.8 Wendland, and *De posteritate Caini* 98 = 2.21.10 Wendland; etc.);
μελισσονόμοι 'beekeepers' (Aeschylus apud Aristophan. *Ran.* 1274);
ἐλαιολόγοι 'olive-pickers' (Aristophanes, *Ves.* 712);
ἀρτοκόποι 'bread-cooks, bakers' (Xenophon, *An.* 4.4.22, etc.).

(b) Rulers and subordinates 2.5

δικασπόλοι 'cause-handlers, judges' (*Il.* 1.238, etc.); cf. שֹׁפְטֵי אֶרֶץ {šóppeʸ ʔórec} 'national or land-judges' (Is. 40:23, etc. *Landrichter* in German);
βουληφόροι 'counselors' (*Il.* 10.414, *Od.* 13.12, etc.);
δημηγόροι 'public speakers, orators' (Plato, *Leges*, 10.908d, etc.); cf. וְיֹעֲצֵי אֶרֶץ {wyóʕăceʸ ʔórec} 'and counselors of the country' (Job 3:14);
ἀγορᾶνόμοι 'market-tenders, inspectors' (Aristophanes, *Ach.* 824; Lysias 22.16; etc.);
ἀστυνόμοι 'town-commissioners' (Plato, *Leges* 6.763c, etc.);
μετρονόμοι 'measure-inspectors' (Aristotle, *Ath.* 51.2, etc.);
σπονδοφόροι 'libation-bearers, heralds' (Pindar, *Isth.* 2.33, etc.);
μαστιγοφόροι 'whip-bearers, policemen' (Thucydides 4.47.3, etc.);
ἀγγελιαφόροι 'message-bearers' (i.e., Iris and Hermes; Lucian, *Sacr.* 8, etc.);
δασμοφόρος 'tributary' (Herodotus 3.97.1); cf. the plural נֹשְׂאֵי מִנְחָה {noś?éʸ minxóʰ} (II Sam. 8:2,6).

(c) Soldiers 2.6

σακεσφόροι 'shield-bearers' (Euripides, *Phoen.* 139, etc.) = תֹּפְשֵׂי מָגֵן {topśéʸ mogén} (Jer. 46:9, African mercenaries); נֹשְׂאֵי צִנָּה וָרֹמַח {noś?éʸ cinóʰ wórómax} 'shield-and-spear-bearers' (I Chr. 12:24[25]; LXX, **θυρεοφόροι καὶ δορατοφόροι**, cf. 5:18, 12:9); **τοῖς λογχοφόροισιν** 'to the spear-bearers' (Aristophanes, *Pax* 1204, etc.);
μαχαιροφόροι 'sabre-bearers' (Herodotus 9.32.1, etc.; see above, note 20);
γερροφόροι 'wicker-[shield-]bearers' (Xenophon, *An.* 1.8.9, etc.);
κορυνηφόροι 'club-bearers' (i.e., a bodyguard; Herodotus 1.59.5);
נֹשֵׂא כֵלִים {noś? kelíʸm} 'arms-bearer' (sing., I Sam. 16:21);
ἀκοντοβόλοι 'dart-throwers' (Apollonius, *Arg.* 2.1000, etc.);
λιθοβόλοι 'stone-throwers' (Thucydides 6.69.2);

34. **ἀνδροκτόνοι** 'husband-killers' (Herodotus 4.110.1) = Amazons.

35. Similarly יֹשְׁבֵי אֶרֶץ {yóšbeʸ ʔórec} (Is. 24:6); also יֹשְׁבֵי סֶלַע {yóšbeʸ ʕélaə} 'rock-dwellers' (42:11), יֹשְׁבֵי אִי {yóšbeʸ ʔíʸ} 'islanders' (23:2,6), and νύμφαι . . . **ἀγρονόμοι** 'nymphs frequenting the country' (*Od.* 6.105-106).

ἡμεροσκόποι 'daytime watchmen' (Herodotus 7.192.1, etc.);
κελευθοποιοί 'path-makers, scouts' (Aeschylus, *Eum.* 13).

(d) Religious functionaries and purveyors

θυοσκόοι 'sacrificers, priests' *(Il.* 24.221, etc.); 2.7
πυρκόοι· ὑπὸ Δελφῶν ἱερεῖς δι' ἐμπύρων μαντευόμενοι (Hesychius);[36] **πυρφόροι** 'fire-bearers' (Lucian, *Syr.* 42); cf. קֹדְחֵי אֵשׁ {qōdxey ʔéš} 'fire-kindlers' (Is. 50:11);
κανηφόροι 'basket-carriers' (girls; Hermippus, fr. 26.1 Edmonds [**Θεοί**] = sch. Aristoph. *Aues* 1550; Dionysius Hal., *Ant. Rom.* 2.22.2; etc.);
χρησμολόγοι 'soothsayers, oracle-collectors' (Herodotus 7.142.3, 8.96.3, etc.);
θεολόγοι 'theologians' (Aristotle, *Metaph.* 2.4.1000a.9, citing Hesiod as an example; etc.);
ἀστρολόγοι 'star-counters' (Xenophon, *Mem.* 4.2.10; LXX Is. 47:13 = חֹבְרֵי שָׁמַיִם {hoḇréy šomáyim} 'heaven-cutters');
ἀστρονόμοι (Plato, *Rep.* 7.531a, etc.);
ἀγαλματοποιοί 'image-makers, sculptors' (Herodotus 2.46.2; Plato, *Prot.* 311c; etc.) = יֹצְרֵי־פֶסֶל {yòcrey-pésel} (Is. 44:9);
στεφανηπλόκοι 'garland-weavers' (Theophrastus, *H.P.* 6.8.1; Dioscurides, *Mat. med.* 3.75, 4.71; etc.); יוֹדְעֵי נֶהִי {yōwdəey néhiy} 'knowing lament; professional (?) mourners' (Amos 5:16); cf. **θρηνῳδοί** (Hesychius s.v. **Καρῖναι**, female mourners, but masc. in Alciphron 1.36 = 4.9 Schepers[2]), contracted from ***θρηναοιδοί**;
θεοπρόποι 'consulters of God; prophets' *(Il.* 12.228; Herodotus 1.158.2; etc.).

(e) Entertainers

ἑδροστρόφοι 'seat-turners, wrestlers' (Theocritus 24.111); 2.8
γελωτοποιοί 'laugh-makers, jesters' (Xenophon, *An.* 7.3.33; Plato, *Rep.* 10.620c; etc.);
ἰαμβοποιοί 'makers of iambs, lampooners' (Aristotle, *Poet.* 9.1451b.14, etc.);
κωμῳδοποιοί 'comic poets' *(Rhet.* 3.3.1406b.7, *Poet.* 4.1449a.4, etc.);
τραγῳδοποιοί 'tragic poets' (Plato, *Crat.* 425d, etc.);
λογοποιοί 'historians, chroniclers' *(Rep.* 3.391a, etc.);
μυθολόγοι 'story-tellers' (3.392d; Baruch 3:23; etc.);
μετεωρολόγοι 'talking of lofty things' (Plato, *Crat.* 396c, 401b, etc.);
οἰνοχόοι 'wine-pourers, butlers' (Xenophon, *An.* 4.4.22, etc.).

(f) Miscellaneous occupations

οἰκονόμοι 'house-tenders, stewards' (Plato, *Rep.* 3.417a, etc.);[37] 2.9
ἱπποκόμοι 'horse-tenders, grooms' (Xenophon, *Hell.* 2.4.6, etc.);
ὀρεωκόμοι 'muleteers' (5.4.42, etc.);
ἱπποτρόφοι, ἱπποφορβοί 'horse-raisers' (Pindar, *Isth.* 4.14 [3.34]; Xenophon, *Cyr.* 1.1.2; etc.);
ἀργυραμοιβοί 'money-changers' (Plato, *Polit.* 289e; Theocritus 12.37); בְּתֹקְעֵי כָּף {ḇtòqəey-kōp} 'among hand-clappers' (i.e., bargainers; Pr. 22:26);
ποντοπόροι 'sea-farers' (Homer, *Epigr.* 8.1 Monro; the singular עֹבֵר יָם {əōḇér yóm}, Is. 23:2); cf. יוֹדְעֵי יָם {yōwdəey yóm} 'those knowing the sea' (II Chr. 8:18);[38]
οἰακονόμοι 'helmsmen' (Aeschylus, *Pr.* 149);
ὁδοποιοί 'road-makers' (Aeschines 3.25; Josephus, *B.I.* 3.6.2 [117], etc.);
σκηνοποιοί 'booth-makers'[39] (Acts 18:3, etc.);

36. To judge from the gloss, which is all we have, the meaning is not 'fire-kindlers' from κοῖε 'kindle' (Attic κῶε) but 'fire-scanners or -watchers'—diviners of a sort. Cf. οὐ κοῶ 'I don't notice' (Epicharmus, fr. 35.14 [Ἐλπὶς ἡ Πλοῦτος] Kaibel = Athenaeus 6.28[236b]), τὸ κοεῖν ὅ ἐστι νοεῖν (scholium on Aristophanes, *Equ.* 198), and the Latin *caue* 'watch out, beware'; Frisk (above, note 16), I, 890-891, III, 132; Pierre Chantraine, *Dictionnaire étymologique de la langue grecque,* I (Paris, 1968), 551.

37. κληρονόμοι 'estate-keepers, heirs' (Demosthenes 21.20, etc.).

38. עָבְרֵי דֶּרֶךְ {əōḇrey dérek} 'wayfarers' (Lam. 2:15, etc.) = ὁδοιπόροι (Plutarch 8.122). The latter citation, οἱ λίθῳ προσπταίσαντες ὁδοιπόροι, in H. Stephanus, *Thesaurus linguae Graecae,* VI (rev. ed., London, 1824), 6534, remains untraceable. Daniel Wyttenbach, *Lexicon Plutarcheum,* Vol. VIII of his *Plutarchi Chaeronensis Moralia* (Oxford, 1830; repr. Hildesheim, 1962), 1090, 1369, does not help; on his inaccuracy, see the posthumous preface. However, the word ὁδοιπόρος is frequent enough (Theocritus 12.9, etc.), even though I happen not to have found the nom. or voc. pl. in any text.

39. Traditionally rendered 'tent-makers,' but a σκηνή was only in function similar to what is now called a tent. It was an improvised shelter for temporary use. Putting up such structures was a remunerative skill, but the need for them was intermittent; only a large city could have afforded steady employment. So when the emperor Claudius expelled the Jews from Rome, the σκηνοποιοί Aquila and his wife Priscilla took to an itinerant life in Greece and Asia, being joined by Paul (cf. 18:18). Going from place to place in pursuit of opportunities for business, Paul was able to evangelize on the side, every Sabbath.

Αἰγύπτιος πλινθοφόρος 'an Egyptian brick-carrier' (Aristophanes, *Aues* 1133-1134);
ὑλοτόμοι 'wood-cutters' (*Il.* 23.123; Sophocles, *El.* 98; etc.); cf. חֹטְבֵי עֵצִים וְשֹׁאֲבֵי מָיִם {xoʃbéy ɵecíym wšóʔăbey máyim} = LXX, **ξυλοκόποι καὶ ὑδροφόροι** 'wood-cutters and water-carriers' (Joshua 9:21,29);
σκυτοτόμοι 'leather-cutters, cobblers' (*Il.* 7.221; Plato, *Symp.* 191a; etc.);
βαλαντιατόμοι or **βαλαντιοτόμοι** 'cut-purses' (Plato, *Rep.* 8.552d, etc.);
πορνοβοσκοί 'harlot-grazers, pimps' (Aristotle, *E.N.* 4.1.1121b.33, etc.);
βωμολόχοι 'altar-pests' (Pherecrates, fr. 141 Edmonds [*Tyrannis*]; Harpocration s.v. **βωμολοχεύεσθαι**; etc.);
שֹׁדְדֵי לַיְלָה {šōddey láylɔh} 'night-robbers' (Ob. 5);
לֹקְחֵי כֹפֶר {lóqxey kōpɛr} 'ransom-takers' (blackmailers or kidnappers; Amos 5:12).

These last few are disreputable or criminal trades. There are many more pejorative categories of people, like פֹּעֲלֵי אָוֶן {póɵăley ʔówɛn} 'evil-doers' (Hosea 6:8, etc. = **κακοποιοί**, Aristotle, *E.N.* 4.3.1125a.19, etc.).[40] Virtual synonyms are חֹרְשֵׁי אָוֶן {xórše y ʔówɛn} 'evil-plowers' (Job 4:8), חֹשְׁבֵי אָוֶן {xòšbey-ʔówɛn} 'evil-trimmers' (Micah 2:1), שֹׁקְדֵי אָוֶן {šóqdey ʔówɛn} 'evil-watchers' (Is. 29:20), בֹּגְדֵי אָוֶן {bógdey ʔówɛn} 'evil-cloakers' (Ps. 59:6). Although objectively they may not have been professional lawbreakers, the speaker talks as if it were their whole life. Other terms of insult are 2.10

שֹׂנְאֵי טוֹב וְאֹהֲבֵי רָע {són ʔey ʃówb wʔóhăbey róɵ} 'haters of good and lovers of evil' (Micah 3:2);
טֹפְלֵי שָׁקֶר {ʃòpley-šóqɛr} 'men sewing falsehood' (Job 13:4);
דֹּוְבְרֵי שָׁקֶר {dówbrey-šóqɛr} 'speakers of falsehood, liars' (Ps. 63:12) = **ψευδολόγος** (sing., Aristophanes, *Ranae* 1951);
κακολόγοι 'evil-speakers' (Pindar, *Pyth.* 11.28, etc.);
μικρολόγοι 'counting trifles; stingy' ([Demosthenes] 59.36, etc.);
δωροδόκοι 'bribe-takers' ([Plato], *Alc.* 2.150a, etc.) = לֹקֵחַ שֹׁחַד {loqéax šóxad} (Deut. 27:25; pl. not attested);
תֹּעֵי רוּחַ {tòɵey-rúwax} 'astray in spirit' (Is. 29:24).[41]

The function or behavior of animals, tame and wild—and even plants—is also expressed the same way as with men: 2.11

σιτοφόροι τε καὶ σκευοφόροι κάμηλοι 'grain-carrying and baggage-carrying camels' (Herodotus 1.80.2);
ἆθλοφόροι ἵπποι 'prize-winning horses' (*Il.* 11.699; cf. 22.162);
ἵπποι ἀνδροβόροι 'man-eating horses' (Quintus, *Posthom.* 6.246-247);
כְּזֹאֵבִים טֹרְפֵי טָרֶף {kizʔebíym ʃórpey ʃórɛp} 'like flesh-tearing wolves' (Ezek. 22:27);
φοίνικες . . . καρποφόροι 'fruit-bearing palm-trees' (Herodotus 1.193.4, etc.); cf. וְעֵץ עֹשֶׂה פְּרִי {wɵéc ɵòssɛh-prí y} 'and fruit-bearing tree[s]' (Gen. 1:12).

Through an oversight that puzzles and troubles me, I left out of my book a morphological parallel closely related to the plural [CóC°/ey ~ CoC^iC°/éy] but even neater. In the singular it is [CóCe], with matching vowels in both syllables. In Greek it is limited to the vocative case; e.g., **βουκόλε** 'cowherd' (*Od.* 21.189),[42] the nominative being **βουκόλος** (*Il.* 23.845, etc.), whereas in the plural **βουκόλοι** can serve either as vocative or as nominative.[43] Hebrew, while devoid of case distinctions, has a differ- 2.12

40. κλῶπες κακοῦργοι 'evil-doing thieves' (Herodotus 1.41.2).
41. Contrariwise, a few terms of praise:
שֹׂנְאֵי בֶצַע {són ʔey bócaɵ} 'pelf-haters' (Ex. 18:21);
רֹדְפֵי צֶדֶק {ródpey cédɛq} 'pursuing honesty' (Is. 51:1);
לְהֹלְכֵי תֹם {lhólkey tom} 'to men walking aright' (Pr. 2:7); cf. ὀρθοπόρου (Porphyry, *Plot.* 22.37).
42. Similarly αἰπόλε 'goatherd' (Theocritus 1.1,12, *Ep.* 4.1), βουφόνε 'cow-killer' (*h. Merc.* 436), μιαιφόνε 'murderer' (*Il.* 5.31,455), βουληφόρε 'counselor' (5.180, etc.), βατοδρόπε 'bramble-reaper' (*h. Merc.* 190), ὁδοιπόρε 'wayfarer' (Theocritus 23.47, *Ep.* 15.2), γεωπόνε 'land-worker, farmer' (Antiphilus and Heraclides in *Anth. Pal.* 7.175.1,281.1), σκυτοτόμε 'cobbler' (Aristophanes, *Lys.* 416), χρῦσοχόε 'goldsmith' (408; χρῦσοχόον, *Od.* 3.425).
43. The instances that I have found happen to be nominative: *Il.* 13.571, *Od.* 11.293; Herodotus 2.164.1; Euripides, *Bac.* 714; Xenophon, *Cyr.* 1.1.2; Plato, *Tim.* 22d; Aristotle, *Hist. an.* 9.4.611a.8-9; Theocritus 25.151; Philo, *De sacrificiis Abelis* 104 = 1.245.5 Cohn, etc.; indeterminate in Cratinus, fr. 46 Edmonds. But κοπρολόγοι 'dung-gatherers,' vocative in Aristophanes, *Pax* 9, nominative in Aristotle, *Ath.* 50.2, etc., illustrates the formal identity of voc. and nom. in the plural.

ent limitation: [–e] will occur only after a biconsonantal root, as in עֹשֵׂה פֶּלֶא {ʕóśeʰ pélɛʔ} 'wonder-worker' (Ex. 15:11, addressed to the LORD in a very archaic verse;[44] also 77:15).[45] A triconsonantal verb will have an interior [–e–] unlike Greek: שֹׁלֵף חָרֶב {šólèp xóreb} 'sword-drawing' (Judges 20:15,17, etc.; cf. the plural above, §2.2).[46]

2.13 The structure [CóCe], shared by the two languages, is most important for being an immediate derivative from the root in its simplest vocalization [CECe].[47] As the imperative singular is the most fundamental use of the verb, so the single change of the vowel within the root to [o] marks the smallest step from verb to noun; e.g., **φέρε δεῦρο ταύτην τὴν ὑδρίαν, ὑδριᾰφόρε** 'Bear that pitcher here, pitcher-bearer' (Aristophanes, *Eccl.* 738). This noun is a man doing so-and-so; and in the natural use of languages the primary occasion for words designating a person is to call him.[48] So the correspondence of the **–κόλε, –φόρε** type in Greek to the {ʕóśeʰ} in Hebrew is not a peripheral but a central matter, touching the essential and highly practical sphere of communication: what each one does and what he is in the mouth of his fellows.

2.14 Many of these terms doubtless originated in the historical period, not much earlier than the author who happens to provide us with the first attestation. But the pattern, the type of compounding, must have been well established in prehistoric Hebrew and Greek. We find {ʕóśeʰ pélɛʔ} 'wonder-worker' in one of the oldest specimens of Hebrew, while {róʕeʸ cóʔn} 'herdsmen' designates the primeval livelihood of the Israelites before they settled down in one country. Of the Greek terms cited above, a sizable minority are already known from the Homeric poems. Several have been detected by the Mycenologists in Linear B tablets, notably
{qo-u-ko-ro} (PY An18.9, Nn831.5; cf. **βουκόλοι** 'cowherds');
{a-to-po-qo} (MY Au102.14, Oe117, PY Fn50.7, An39.11, 427.3; cf. **ἀρτοκόποι** 'bakers,' above, §2.4);
{du-ru-to-mo} (PY Vn10.1; cf. **δρυτόμος** 'tree-cutter,' *Il.* 11.86, 23.315, etc.);
{to-ko-do-mo} (PY An7.11, 18.6, 35.1; cf. **τοιχοδόμος** 'wall-builder,' Dionysius Alexandrinus apud Euseb. *Praep. euan.* 14.24 [774a = 21.1273a Migne]);
{re-wo-to-ro-ko-wo} (PY Aa783, Ab553, Ad676; cf. **λοετροχόῳ** 'bath-pourer,' *Od.* 20.297; **λουτροχόους,** Xenophon, *Cyr.* 8.8.20).
It may be significant that none of these is recorded at Knosos in Crete, but only on the mainland.[49] The transcription of most of the Linear B characters in these words is pretty well founded, in my judgement;[50] but on the other hand the values {u, dù, mo} remain quite conjectural and unconfirmed. Nothing in the brief context of the tablets shows that the meaning is indeed 'cowherds,' 'bakers,' etc. At most the neighboring ideogram of a MAN on several tablets proves that the interpretation of the words as occupational terms is not contradicted.[51]

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44. Levin (above, note 33), p. 322. Cf. τὸν τερατοποιὸν Κύριον in II Macc. 15:22, and τοῖς θαυματοποιοῖς 'for the puppeteers' (Plato, *Rep.* 7.514b).

45. The same structure occurs in other passages where a Greek translation would have to be nominative; e.g., רֹעֶה צֹאן {róʕeʰ cóʔn} 'shepherd, goatherd' is the subject in Gen. 46:34 and the predicate in 4:2, syntactically equivalent to αἰπόλος (*Il.* 4.275, Theocritus 7.13, etc.) although matching αἰπόλε morphologically. In Jer. 4:29, "from the sound of horseman and bow-shooter" (וְמֵהֹמֶה קֶשֶׁת {wəróme^h qéšɛt}), the LXX of course has a genitive; a morphological parallel is ἑκατηβόλε 'far-shooter' (*Il.* 15.231).

In עֹטֶה־אוֹר כַּשַּׂלְמָה {ʕóṭeʰ-ʔóʷr kaśalmóʰ} 'wearing light like a garment' (Ps. 104:2) the Hebrew participle could be construed as equivalent either to the Greek vocative φωσφόρε 'light-bearer or -wearer' (Euripides, *Hel.* 569; φαεσφόρε, Callimachus, *In Dianam* 204) or to the nominative φωσφόρος (Euripides, *Ion* 1157; Aristophanes, *Ranae* 342; etc.). Also תֹּלֶה אֶרֶץ {tólɛʰ ʔérɛc} 'hanging the earth' (Job 26:7) could be rendered by either a vocative or a nominative in Greek.

46. And often that [e] rather than the [o] will be accented, notwithstanding another accent at the beginning of the next word: יֹשֵׁב אֹהֶל {yošéb ʔóhɛl} 'tent-dweller' (Gen. 4:20).

47. Levin (above, note 33), pp. 367-368, 381-384, 455-471.

48. The written texts through which we have access to ancient Greek are on the whole narrative or discursive rather than conversational, and therefore show such nouns more often in the nominative case than in the vocative, with exceptions such as Σώκρατες in Plato's and Xenophon's dialogues. In daily speech the vocative was bound to be more prominent.

49. {–u–ko–ro}, however, appears on two fragmentary tablets (KN As6067.2, X5610).

50. *The Linear B Decipherment Controversy Re-examined* (State University of New York, 1964), pp. 64-155. The majority of researchers in the Mycenaean field accept, virtually *in toto*, Ventris's key to the Linear B syllabary; but the larger profession of Hellenists is not so thoroughly convinced. See E. A. Barber (ed.) in the preface to the *Supplement* (Oxford, 1968) to Liddell-Scott-Jones, *Greek-English Lexicon.*

51. In Aa783 the ideogram stands for a **WOMAN**.

3. πρᾶγος, πρᾶγμα, πρᾶξις (–ῆ–) 'business' or 'trade'

My article on "'Something Stolen': a Semitic Participle and an Indo-European Neuter Substantive"[52] shows the Hebrew {–ḡneḇoṯ–} and the Greek κλέπος to be cognate in their formation as well as their root. The same structure is well represented by many other nouns of passive or stative meaning from triconsonantal roots, some Semitic and others Indo-European. The Indo-European languages also have a synonymous formation, exemplified by κλέμμα. In general the –μα nouns are more frequent than the –ος. 3.1

One other Greek noun that has a likely Hebrew cognate is of similar formation, but from a root of more than three consonants: πρᾶγος, used somewhat sparsely by Pindar (*Nem.* 3.6, fr. 108a. 2; [π]ΡΑΓΕΣΙΝ, *Dith.* 4.21 Snell = *POxy.* 13.2445) and the Attic poets (Aeschylus, *Septem* 599, πρᾱ́γει, dative; Sophocles, *Ajax* 347, πρᾱ́γη, pl.; etc.). πρᾶγμα is more common, and also is found copiously in prose (Plato, *Apol.* 20c, etc.). The Ionic counterpart is πρῆγμα (Herodotus 1.19.1,32.4, etc.) or ΠΡΗΧΜΑ (Schwyzer 688B.16-17, C.5, Chios, 5th century B.C.;[53] alternate spelling in Herodotus 3.49.2,57.2 according to *POxy.* 13.1619.326,379; πρηχμάτων in 1.133.3 as quoted by Athenaeus 4.144a; ΠΡΗΧΜΑΤΟΣ, *Epigr. Gr.* 1092.2 Kaibel). *πρῆγος or *πρῆχος is not attested; for poetry in Ionic from after the Homeric age is meagerly preserved.[54] Homer uses only πρῆξις (*Od.* 3.82, etc.), in nearly the same sense, but the nuance of the suffix is active: 'doing,' rather than 'something done.'[55] 3.2

Whether the root ends in –γ– or –χ– is ambiguous, not only because of the alternation between πρῆγμα and πρῆχμα, but also because of the verb forms. In the perfect, πέπρᾱγε(ν) 'he has done,' πεπραγέναι 'to have done,' etc., prevail (Pindar, *Pyth.* 2.73; Aristophanes, *Lys.* 462, *Pl.* 633; Plato, *Rep.* 10.603c; etc.); but πεπρηχέναι 'to have done' is found in Herodotus (5.106.4),[56] πέπραχε 'he has done' and ἐπεπράχει 'he had done' in Xenophon (*Cyr.* 3.1.15, *Hell.* 5.2.32), and similarly in many authors thereafter. To be sure, an aspirate that shows up only in the perfect or pluperfect is compatible with a non-aspirate in the root; e.g., πέπομφε(ν) 'he has sent' (Demosthenes 4.48, 8.58, etc.), πεπομφέναι 'to have sent' (Xenophon, *Cyr.* 6.2.10, etc.), ἐπεπόμφει 'he had sent' (6.2.9, etc.), in contrast to πέμπει 'he sends' (7.4.1, etc.), ἔπεμπε(ν) 'he sent' (1.5.4, 4.5.12, etc.), and πομπή 'sending' or 'escort' (*Od.* 11.332, etc.). But the unvoiced –σσ– (–ττ– in strict Attic) of the present and imperfect – πρήσσει, πράττει 'he is doing' (Herodotus 5.106.4; Aristophanes, *Pax* 695, etc.), ἔπρησσε, ἔπραττε(ν) 'he was doing' (Herodotus 4.145.1; Xenophon, *Hell.* 6.3.3; Demosthenes 4.42; etc.) – is normally the Greek reflex of *$\hat{k}$ added to an *unvoiced* plosive.[57] So the πρησσ– or πρᾶττ– forms are more consistent with a root that ends in –χ– than in –γ–. 3.3

The Hebrew noun מְלָאכָה {mlé'ḵeṯ} 'business, occupation, work' (Ex. 35:35, etc.) is phonetically closest to the Ionic *πρῆχε(σ)–, which is unrecorded but equivalent to the Attic πρᾱ́γει (contracted from –εῖ) and πρᾱ́γη (contracted from –εα). The letter χ, in the "psilotic" sub-dialect of Ionic that lacked the sound [h], probably stood for a fricative rather than a strict aspirate [kʰ],[58] and thus was nearly if not quite identical with the Hebrew [ḵ]. The pronunciation *[pré:ḵe(s)–] matches the Hebrew, segment for segment *in all seven segments,* either exactly or approximately. Most striking is the accented vowel; the prolongation of it in Greek is of a piece with the weak consonant א, marked silent by the medieval Hebrew punctators. The initial group of consonants is at least serially related; but the phonology of Greek excludes [ml–] or [mr–], and Hebrew has no non-aspirate [p].[59] The correspondence of Hebrew –מ to Greek π– recurs in מִלְחֶמֶת {milxémeṯ} : πόλεμος (sometimes πτόλεμος in Homer) 'war, battle' (I Sam. 13:22; *Il.* 1.61, 6.328, etc.; cf. above, note 32).[60] Moreover it recurs 3.4

52. To be published in a Festschrift honoring a scholar whose name is now kept confidential in order to surprise him.

53. For a copy in epigraphic capital letters, see B. Haussoullier, "Inscriptions de Chio," *Bulletin de correspondance hellénique,* III (1879), 232.

54. A fragmentary prose inscription from Ephesus has ΤΟΠΡΗΧ at the end of a line; Rudolf Heberdey, "Vorläufiger Bericht über die Ausgrabungen in Ephesus," *Jahreshefte des österreichischen archäologischen Instituts,* II (Beiblatt), 48-49; F. Bechtel et al., *Sammlung der griechischen Dialekt-Inschriften* (Göttingen, 1884-1915), 5598.4. The beginning of the next line has been restored so as to read πρῆχ|[μα], but πρῆχ|[ος] would fit the space just as well. A shadow of uncertainty must remain over either restoration because nobody as yet has proposed a satisfactory word to fill the rest of the damaged fifth line – i.e., the space for four letters after [μα] or [ος].

55. πρᾶξις in Attic (Aeschylus, *Per.* 739; Xenophon, *Cyr.* 2.2; Plato, *Rep.* 4.434a; etc.).

56. πεπρηγέναι in 2.172.5 also means 'to have done,' but in an intransitive construction; i.e., 'to have fared, to have prospered.' The same distinction between the perfect in –χ– and in –γ– is sometimes found in Attic, but no author exhibits it quite so neatly as Herodotus.

57. Schwyzer (above, note 1), I, 714-715.

58. See my article, "Grassmann's 'Law' in the Early Semitic Loan-word χιτών, κ.θών," *Studi Micenei ed Egeo-anatolici,* VIII (1969), 72-74.

59. פ is regularly rendered φ in the LXX and later transcriptions.

60. The etymology of πόλεμος within Indo-European remains unsatisfactory; Frisk (above, note 16), II, 575.

in the clearly cognate and basic root that means 'full,' {m–l–ʔ} in the Semitic languages; the most extensive correspondence is in מְלֵאתִי מִשְׁפָּט {mleʔátiʸ mišpóʃ} 'full of justice' (fem. adjective in agreement with קִרְיָה {qiryóʰ} 'town'; Is. 1:21) : **πλησιφαής** 'full of light' (describing the moon; Philo, *De congressu eruditionis gratia* 19[106] = 3.93.20-21 Cohn and *De specialibus legibus* 2.19[155] = 5.123.7 Cohn, etc.; the **η** in **πλη–** is not confined to Ionic as in **πρηγ/χ–** but is pan-Hellenic and has Latin cognates such as *plēnus* 'full,' *complētus,* etc.).

{mlé^{ʔ}kəṯ} is limited to the "construct" function: '*someone's* business.' The "absolute" 3.5
מְלָאכָה {mlɔʔkóʰ}, simply 'business' (Ex. 31:15, etc.), is much farther from the Greek. In nouns from a triconsonantal root, the Greek and Indo-European formation – such as **κλέπτος** – corresponds likewise to the Semitic "construct" rather than the "absolute." The "construct" takes a special form in Hebrew when an accented pronominal suffix becomes part of the same word: מְלַאכְתּוֹ {mlaʔkt|ɔ́ʷ} 'his business' (Gen. 39:11, etc.), מְלַאכְתֶּךָ {mlaʔkt|éko} 'your business' (Ex. 20:9 = Deut. 5:13, etc.); *מְלַאכְתִּי *{mlaʔkt|íʸ} 'my business' happens to be unrecorded in the Bible. The vowel [a], along with the letter א, is reminiscent of the non-Ionic **ᾱ**; but otherwise the structure diverges greatly from **πρᾶγος, πράγε(σ)–**. It is much more like **πρᾶξι|ς** (see above, §3.2 and note 55); and especially the Greek personal names that begin with **Πρᾶξι–**, or **Πρᾶξ–** before a vowel, can be interpreted 'My-business-[is]–'; e.g.; **ΠΡΑΧΣΙΒΟ[λ]ΟΣ** (in the old Attic alphabet = **Πρᾶξίβουλος**; *IG* 1.260.2 = *IG*^{2} 1.220.3) 'My-business-[is-the-]council' or '[is-good-]counsel,' **Πρᾶξαγόρᾱ** 'My-business-[is-the-]public-gathering' (Aristophanes, *Eccl.* 124, etc.).[60a]

Dictionaries derive {mlɔʔkóʰ} and its "construct" {mlé^{ʔ}kəṯ, mlaʔkt–} from a root 3.6
{l–ʔ–k}; but no such verb is found in Hebrew. In Ugaritic it exists and means 'send';[61] and the two languages unquestionably share the noun {mlʔk} : מַלְאָךְ {malʔóḵ} 'messenger' (Num. 20:16, I Kings 19:2, etc.).[62] The other Ugaritic noun {mlʔkt} 'message' matches better the sense of מְלָאכוּת {malʔăkúʷt} (Hag. 1:13) than of {mlé^{ʔ}kəṯ}. In form it could agree with either, so far as we can judge Ugaritic in the absence of vocalization. While {malʔóḵ} can be understood in many passages as 'deputy' and hence 'agent,' the semantic link to {mlɔʔkóʰ, mlé^{ʔ}kəṯ} is tenuous and questionable. The latter noun occurs upwards of 150 times in the Bible, but never in a context along with {malʔóḵ} or referring to what a {malʔóḵ} does. It does not signify 'a message,' nor even 'a mission' that one has been sent upon as an agent for a principal who stays behind awaiting the result. At most the phrase מְלֶאכֶת הַמֶּלֶךְ {mlé^{ʔ}kəṯ ham·élek} (I Chr. 26:30, Dan. 8:27) may equal 'the king's charge' – hence 'the king's business.'

I think it likely that this word came from a non-Semitic source and had its beginning conson- 3.7
ants modified by cross-influence from the Semitic {malʔóḵ}. For it is limited to Hebrew and the neighboring dialect, Phoenician.[63] A verb {l–ʔ–k} 'send' coexists nowhere with a noun allegedly derived from it but meaning 'work' or 'business.' [mCVCC–] is still a very odd pattern in Hebrew; a further assimilation or Semitization appears in the "Babylonian" text of the Bible: מֹלֹאכֹת, מֹלֹאכֹת, standing

60a. Lev. 23:36 forbids any work or business {mlé^{ʔ}kəṯ} that is servile on the day of עֲצֶרֶת {əăcéreṯ} 'public gathering,' which is an alternant of עֲצָרָה {əăcoróʰ} (Joel 1:14, 2:15, II Kings 10:20). The latter is very close to **ἀγορᾱ́** (**ἀγορή** in Ionic); see Levin (above, note 33), pp. 262 ff. The passage in Leviticus helps to pinpoint the honorific quality of the Greek name **Πρᾶξαγόρᾱ** (**–ᾱς** if masc.), which implies, "My work commands the notice of the *assembled* multitude."

61. Joseph Aistleitner, *Wörterbuch der ugaritischen Sprache,* 2d ed. by Otto Eissfeldt (Berichte über die Verhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig, Philologisch-historische Klasse, Band 106, Heft 3; Berlin, 1965), pp. 165-166.

62. Translated **ἄγγελος** in Greek, and secondarily *nuntius* in Latin unless he is from God – in which case the Greek term is not translated but merely transcribed *angelus.*

63. H. Donner – W. Röllig, *Kanaanäische und aramäische Inschriften,* 2d ed. (Wiesbaden, 1966-9), I, 2, 8; III, 14. Phenician differs from Hebrew in having no "absolute" מלאכה (or any other feminine with the ending ה–); מלאכת {mlʔkt} serves for the "absolute" function, as in one Hebrew passage (II Chr. 13:10). The inscriptions from the western or Punic region usually show the spelling מלכת {mlkt}.

for the sequence [mælʔɔk̄ɔ, mælʔæk̄æt̄][64] – just as if it were the feminine of מלאך 'messenger.'[65]

Unlike {mlέʔk̄ɛt̄}, the Greek equivalent πρᾶγος or *πρῆχος, *πρήχε(σ)– is unmistakably related to a verb, although neither the noun nor the verb has Indo-European cognates. The root 3.8
πρᾱ/ηγ– or πρᾱ/ηχ– has been analyzed as an extension of the Indo-European *per–, *pr– in two steps.[66] The basic meaning 'cross' is evident in the Latin preposition *per* and in the Greek transitive verb 'send across; trade, sell': πέρνασχ' ὅντιν' ἕλεσκε πέρην ἁλὸς ἀτρυγέτοιο / ἐς Σάμον 'he used to trade whomever he captured across the barren sea to Samos [= Samothrace]' (*Il.* 24.752-753; cf. the active participle περνᾶς, 22.45, and the middle-passive participle περνάμενα, 18.292). Beyond the biconsonantal minimum of the root, one extension is περᾱ/η–, as in πέρησε 'it passed, crossed, traversed' (4.460, etc.). Nearly the same sense, alongside of 'do,' appears in the doubly extended root we have been studying: ἰθύει ἀλλ' οὐ πρήσσει 'he heads [for the cattle] but does not get through' (11.552, 17.661), ἅλα πρήσσοντες 'traversing the sea' (*Od.* 9.491). This meaning is limited to the present (and imperfect), and found only in certain Homeric passages. But elsewhere in the same poems, through figurative usage (like *making headway* in English) the meaning comes down virtually to 'do,' with the implication of something gained: πρῆξαι δ' ἔμπης οὔ τι δυνήσεαι 'still you won't be able to do anything [= get anywhere]' (*Il.* 1. 561). That is also perceptible in the derived nouns πρῆξις, πρηκτήρ 'dealer, trader' (*Od.* 8.162), πρῆγμα (πρῆχμα), and all the corresponding non-Ionic forms with –ᾱ–, including πρᾶγος. It is furthermore in the Hebrew {mlέʔk̄ɛt̄}.

If {malʔɔ́k̄} 'messenger, deputy' is indeed from a triconsonantal root {l–ʔ–k} lacking in Hebrew but current in Ugaritic and Arabic, its morphological structure is abnormal even so. There 3.9
is no other {maCCɔ́C} with [a] before the first of the three radical consonants, unless that consonant is guttural and therefore favors a wide-open vowel;[67] nor does any noun of roughly similar formation designate a man.[68] But {malʔɔ́k̄} has the same vowels as רַכָּב {rak·ɔ́b̄} 'charioteer' (II Kings 9:17), סַבָּל {ʃab·ɔ́l} 'porter' (II Chr. 2:1, etc.), גַּנָּב {gan·ɔ́b̄} 'thief' (Jer. 2:26, etc.), and other occupational terms formed from triconsonantal roots with strengthening or gemination of the middle consonant.[69] The four consonants of מלאך are treated as though they constituted a root, with [a] between the first two and [ɔ́] between the last two.[70]

{mlέʔk̄ɛt̄} with its Greek counterpart πράγε(σ)– or *πρήχε(σ)– is conspicuous for length. 3.10
The vocabulary items common to the two languages are on the whole short, if we leave out some manifest borrowings, doubtless less ancient.[71] This item of vocabulary bespeaks a recognition by both

64. Paul Kahle, *Der masoretische Text des Alten Testaments nach der Überlieferung der babylonischen Juden* (Leipzig, 1902; repr. Hildesheim, 1966), p. 73. The precise quality of the "Babylonian" vowels is undetermined; for the supralinear notation went out of use some centuries ago.

65. According to G. W. Freytag, *Lexicon Arabico-latinum*, IV (Halle, 1837), 77, and A. de B. Kazimirski, *Dictionnaire arabe-français* (Paris, 1860), II, 952, the masc. مَلَاكٌ {malʔak|un} is either 'messenger, angel' or 'message, mission,' while the fem. مَلَاكَةٌ {malʔakat|un} is only the latter.

66. Frisk (above, note 16), II, 590-591; Leo Mayer, "Θεοπρόπος," *Zeitschrift für vergleichende Sprachforschung*, XXII (1874), 61-64.

67. מַאֲכָל {maʔăk̄ɔ́l} 'food' (Gen. 6:21, etc.); הַמַּעֲרָב {ham·aəărɔ́b̄} 'the sunset, the west' (Dan. 8:5).

68. E.g., מִשְׁפָּט {mišpɔ́ʃ} (Deut. 16:19, etc.) means 'judgement.' The causative passive participle, such as מֻשְׁכָּב {muškɔ́b̄} '(having been) laid' (II Kings 4:32), must have a back-vowel in the prefix.

In Arabic also, {malʔak|un} (see above, note 65) does not fit any model of a noun designating a man; Wolfdietrich Fischer, *Grammatik des klassischen Arabisch* ("Porta Linguarum Orientalium," n.s. XI; Wiesbaden, 1972), pp. 38, 45-46, 110. The anomaly may have been what prompted the odd creation of a by-form مَلَكٌ {malak|un} in the sense of 'angel'; for it then becomes like a triconsonantal root with normal vocalization.

69. They are enumerated in Hans Bauer and Pontus Leander, *Historische Grammatik der hebräischen Sprache des Alten Testaments*, I (Halle, 1922; repr. Hildesheim, 1962), 479.

70. Likewise a verb with a four-consonant root, יְכַרְסְמֶנָּה {yk̄arʃmén·ɔʰ} 'he will ravage her' (Ps. 80:14), is conjugated like a triconsonantal of the intensive conjugation: יְשַׁלְּחֶנּוּ {yšal·xén·uʷ} 'he will send him off' (Ex. 21:26, etc.).

71. E.g., the commercial term עֵרָבוֹן {əerɔb̄óʷn} : ἀρραβών 'a pledge, an earnest' (Gen. 38:17; Isaeus 8.23, etc.); Émilia Masson, *Recherches sur les plus anciens emprunts sémitiques en grec* (Études et commentaires LXVII; Paris, 1967), pp. 30-31.

peoples of the work or business that takes up a man's days. On the whole it is skilled or specialized, not the same as that of every other man in the society. So it marks an important stage of civilized development, well beyond the primitive "jack-of-all-trades" who of necessity applied his own hands to whatever had to be done for a living.

4. Hereditary occupations of women and gentilic status

Plato (in *Prot.* 347d) has Socrates remark, "When boon-companions are gentlemen and educated, you won't see either pipers or dancers or harp-players." The three professions, in Greek, are **οὔτ' αὐλητρίδας οὔτε ὀρχηστρίδας οὔτε ψαλτρίας.** The last one has a different though somewhat related suffix; in the nominative singular they would be **αὐλητρίς, ὀρχηστρίς, ψάλτρια** (Xenophon, *Symp.* 2.1.11). A clue to the distinction between **–τρίς** and **–τρια** lies in the history of the related masculine nouns. Whereas **αὐλητρίς** and **ὀρχηστρίς** first appear in comic poetry of the fifth century B.C. (Crates, fr. 27 Edmonds [**Σαλαμίνιοι** = **Σάμιοι?**] = Erotian 103.13 Nachmanson; Aristophanes, *Ach.* 551, 1093),[72] **ὀρχηστήρ** and **αὐλητήρ** are attested as early as Homer and Hesiod *(Il.* 18.494, *Sc.* 283; also **ὀρχηστής,** *Il.* 16.617, 24.261). ***ψαλτήρ** on the other hand does not occur, and **ψάλτης** turns up around the same period as **ψάλτρια.**[73] 4.1

I infer that **αὐλητρίς** originally meant 'piper's daughter,' and **ὀρχηστρίς** 'dancer's daughter.' I.e., there were already men piping and dancing; and the first girls to perform learned how from their fathers. Or so it seemed to the audience. Doubtless they were taught in childhood; and if they kept at it when grown up, that was no reason to call them by a different term. 4.2

Some time after these feminine terms had become established in the language, there was room for a social development such that a girl could learn the art outside of her family. Thus, in Xenophon's *Symposium,* which is set in the latter part of the fifth century (8.39, when Pericles was no more), the Syracusan **ὀρχηστροδιδάσκαλος** 'dancers' coach' (2.15, 9.3) exhibits a small troupe of performers including an **αὐλητρίς,** an **ὀρχηστρίς,** and a boy who plays the lyre and dances. They are a quasi-family, in that Socrates addressing the man speaks of **ὁ μὲν παῖς ὅδε ὁ σὸς καὶ ἡ παῖς ἥδε** (the dancing girl, 7.2); but 'your kid' (masc.) is described in an earlier passage not as though he were a son but as the man's homosexual pet (4.52-54), and presumably neither is the girl his daughter.[74] That does not make the suffix **–τρίς** incongruous. For besides the lingering similarity to a team of father and daughter or daughters, the morphological sense of **–τρίς** was hardly obtrusive. The Ablaut relation between **–τήρ** and **–τρ–** lay far back in the past, so that **–τρίς** was not readily analyzed as 'daughter of **–τήρ.**' The **–ίς** part too had ceased to be a straightforward formant of patronymics, as in Homer's **Χρῡσηΐς,** the daughter of **Χρῡσης** *(Il.* 1.370,439,450), and **Βρῑσηΐς** (19.282, 24.676).[75] 4.3

The other suffix **–τρια** evinces a similar Ablaut in contrast to **–τήρ,** but never suggested that the woman doing such-and-such was the daughter of a man in the same line of work:[76] **πενθητήρ, πενθήτρια** 'mourner' (Aeschylus, *Septem* 1062, *Per.* 946; Euripides, *Hipp.* 805); **τρυγητήρ, τρυγήτρια** 'harvester' (Hesiod, *Sc.* 293; Demosthenes 57.45). The earliest occurrence of **–τρια** is in Archilochus, **ξείνων δέκτρια** 'hostess' (fr. 19.2 Bergk = Athenaeus 13.594d; cf. **ξεινοδόκοι** above, §2.2). **ἀλετρίς** 'miller woman' *(Od.* 20.105) is presumably somewhat older.[77] But in most periods **–τρίς** was less productive than **–τρια.**[78] The new formations in **–τρίς,** like the more plentiful ones in **–τήρ,** referred chiefly to tools or utensils, leaving the field of persons to **–τρια** and **–της;**[79] e.g., **εἰσοπτρίς** 'mirror' (Phanias in *Anth. Pal.* 6.307.2), **μήλη ὑπαλειπτρίς** 'an anointing probe' (Hippocrates, *Superf.* 28), **φυσητήρ** 'blow-pipe' (Herodotus 4.2.1, etc.), but **ἀγύρτρια** and **ἀγύρτης** 'beggar, vagabond' (Aeschylus, *Ag.* 1273; Euripides, *Rh.* 503, 715, etc.).[80] 4.4

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72. An epigram *(Anth. Pal.* 5.159) with **ηὐλητρίς** 'the piper' is attributed by the mss. to Simonides (fr. 178 Bergk); but editors consider it to be by a much later poet.

73. Ion of Chios, fr. 22 Snell [*Omphale*] = Athenaeus 14.634f; Menander, fr. 495 Edmonds [**Ὑποβολιμαῖος**] = Quintilian 1.10.18; Machon apud Athen. 8.348f. Also [ψ]ΑΛΤΑΣ in *PHibeh* 1.15.7, conjecturally attributed to the sophist Hippias of Elis.

74. In Terence's *Phormio* the art is still less something transmitted within the family; the pimp who owns the *citharistria* sends her to school to learn it (82-88).

75. In the accusative case both **Βρῑσηΐδα** and **κούρην Βρῑσῆος** are used (1.184,392, etc.).

76. Buck-Petersen (above, note 26), pp. 154-156, 426-427; Ernst Fraenkel, *Geschichte der griechischen Nomina agentis auf* –τήρ, –τωρ, –της (–τ–), I (Strassburg, 1910), 225.

77. The only masc. counterpart is **ὄνους ἀλέτας** 'millstones,' literally 'grinder asses' (acc. pl.; Xenophon, *An.* 1.5.5); the sing. ΟΝΟΝΑΛΕΤΑΝ (M. Guarducci, *Inscriptiones Creticae* 4.75B.7). In the context of the *Odyssey* the **ἀλετρίς** has a hard, servile lot; twelve women do the work by their own sheer strength. That some if not all of them were born into bondage, is likely; but we can only speculate whether there was ever any variety in the custom, so that the miller was a man (***ἀλετήρ**) assisted by a real ass and employing his daughters as helpers.

78. **–ιᾰ** is not the feminine of **–ιος,** which does sometimes bear a patronymic sense, as in **Τελαμώνιος** 'Telamon's son' (see above, §1.2).

79. P. Chantraine, *La formation des noms en grec ancien* (Paris, 1933; Collection Linguistique XXXVIII), pp. 340-341.

80. The interchange of **–τήρ** and **–της** attached to the same base is common enough, as in **ὀρχηστήρ, ὀρχηστής.**

The Indo-European character of **–τήρ, –τρ–** is well established, and **–ια** too corresponds regularly to the Sanskrit {–ī}, the Lithuanian –ī, etc.[81] But the etymology of **–ίς (–ίδ–)** remains up in the air. That it was a suffix of *patrilinear identification,* is plain in many of the early occurrences; e.g., **Δαρδανίδες** (*Il.* 18.339) from **Δάρδανος** (20.219, 304), and **Δαναΐδες** (genitive **Δαναΐδων**, Euripides, *Phoen.* 1675)[82] from **Δαναός** (Aeschylus, *Sup.* 12, 321, 979; etc.). In this ethnic function, which is frequent, it is most reminiscent of the Hebrew {–ī^y^t}: אִשָּׁה יִשְׂרְאֵלִית {ʔiš^ɔ^h yisrʔelí^y^t} 'an Israelite woman' (Lev. 24:10), but in the plural בְּנוֹת יִשְׂרָאֵל {bnó^w^t yisroʔél}, literally 'Israel's daughters' (Judges 11:40, II Sam. 1:24); and again חִתִּית {xit·í^y^t} 'Hittite [woman]' (Ezek. 16:3,45), but בְּנוֹת חֵת {bnó^w^t xét} 'Heth's daughters' (Gen. 27:46);[83] הַכְּנַעֲנִית {haknaəăní^y^t} 'the Phoenician or Canaanite [woman]' (Gen. 46:10, Ex. 6:15, I Chr. 2:3),[84] but בְּנוֹת כְּנָעַן {bnó^w^t knóəan} 'Canaan's daughters' (Gen. 28:8). The Hebrew authors treated nationality as a heritage from a patriarch and often used the explicit word {bne^y^} 'sons' or {bno^w^t} 'daughters' in that context to designate the people as a whole. An individual, however, was expressed by the suffix {–ī^y^} (see above, §1.10-12), or {–ī^y^t} if feminine.[85] 4.5

In Greek, virtual patronymics were generally preferred to explicit ones, as we have seen in the example of **Βρῖσηΐς** (10 times, including case-forms), **κούρην Βρῖσῆος** (3 times).[86] But for nations the virtual patronymics appear very little in the masculine (**Δαρδανίωνες,** (*Il.* 7.414, 8.154).[87] Instead the Homeric norm is either a plural without patronymic suffix – **Δάρδανοι** (10 times), **Ἀχαιοί** (hundreds of times) – or else, somewhat less copiously, the genitive plural of the same preceded by 'sons' – **υἷες Ἀχαιῶν** (*Il.* 1.162, etc.).[88] An individual is **Δάρδανος ἀνήρ** (2.701, 16.807), **Ἀχαιὸς ἀνήρ** (3.167,226); so little is he distinguished from the eponymous patriarch. But it is different in the feminine: **Δαρδανίδες, Ἀχαΐδες** (2.235, 7.96, 9.395; *Od.* 21.251) with the patronymic suffix.[89] That is logical because their nationality was inherited from their fathers, rather than imparted by their husbands upon marriage. For most individuals it would not have mattered practically, because they married within their own nation anyhow; but exogamy[90] was allowed to some extent, and the nationality of a prospective bride was a consideration (cf. *Il.* 9.395). 4.6

One ethnic, meaning 'Pelasgian or Philistine woman,' was probably shared by Greek and Hebrew, although the Hebrew feminine happens not to be attested. The nationality of the infamous Delilah is not expressed but only implied (Judges 16:4 ff.). **Πελασγίς** is recorded, in the genitive plural **τῶν Πελασγίδων γυναικῶν** (Herodotus 6.138.2) describing the non-Greek inhabitants of Lemnos. The women of that breed were just as notorious for murdering their husbands as Delilah for betraying hers. The variation **Πελασγ–/Πελαστ–** (also **Πελαργ–**) is found in a scholium on **Ζεῦ ἄνα Δωδωναῖε Πελασγικέ** (*Il.* 16.233). On the Hebrew side *{plišti^y^t} to match ***Πελαστίς** can be inferred from בְּנוֹת פְּלִשְׁתִּים {bnó^w^t plišti^y^m} 'Philistines' daughters' (II Sam. 1:20, Ezek. 16:27,57) and הַפְּלִשְׁתִּי {hap·lišti^y^} 'the Philistine [man]' (I Sam. 17:8, etc.). The identification of the Pelasgians with the Philistines is 4.7

But rarely do –τρίς and –τρια alternate in that fashion. αὐλήτρια in Diogenes Laërtius 7.62 was formed artificially just to resolve the ambiguity in his example of αὐλητρίς as one word or two (αὐλὴ τρίς): Αὐλητρὶς πέπτωκε· δηλοῦται γὰρ δἰ' αὐτῆς τὸ μὲν τοιοῦτον, Οἴκἷα τρὶς πέπτωκε, τὸ δὲ τοιοῦτον, Αὐλήτρια πέπτωκε.

81. Schwyzer (above, note 1), I, 473.

82. Δαναΐδες is the title of a lost play of Aeschylus, belonging to the same trilogy as the *Suppliants.*

83. Also חִתִּית {xit·iy·ót} 'Hittite [women]' (I Kings 11:1).

84. Translated τῆς Φοινίσσης in Ex. 6:15, but τῆς Χανα(α)νίτιδος in the other two passages.

85. More rarely {–iy·ó^h^}, as in הַמּוֹאֲבִיָּה {ham·o^w^ʔăbiy·ó^h^} 'the Moabite [woman]' (Ruth 2:21, etc.); but הַמּוֹאֲבִית {ham·o^w^ʔobiy^t^} in a different book (II Chr. 24:26).

86. Similarly, in the *Iliad* and the *Odyssey,*

Ἀτρείδη	49			Ἀτρέος υἱέ	6
Ἀτρεΐδης	78			Ἀτρέος υἱός	1
Ἀτρεΐδην	16	Ἀτρεΐωνα	3	Ἀτρέος υἱόν	7
Ἀτρείδῃ	14	Ἀτρεΐωνι	1		
Ἀτρείδαο	27	Ἀτρεΐωνος	1		
Ἀτρείδεω	7				

87. Euripides was fond of Δαναΐδαι (*I.A.* 352, etc.); see James T. Allen and Gabriel Italie, *A Concordance to Euripides* (Berkeley, 1954), p. 136. But Homer has only Δαναοῖ in reference to the same people (*Il.* 1.42, etc.).

88. The hexamater would not accommodate Δαρδάνων (Pindar, *Nem.* 3.62).

89. Besides Ἀχαιαί in the *Odyssey* (2.119, 19.542), which is just the "first declension" derivative from Ἀχαιοί and not a patronymic.

90. This is a neo-Hellenic word for what was actually called ἐπιγαμία by the Greeks (Xenophon, *Cyr.* 3.2.23, etc.).

tolerably sure,[91] and gives us some inkling of how the suffix {-ĭ^y t} : **-ίς, -ίδ-** could have diffused around the eastern Mediterranean, although the language of that people is lost.

The pairing of masculine **-τήρ** and feminine **-τρίς** is paralleled morphologically by **-της, -τις**: **ἱκέτης** 'suppliant' (*Od.* 5.450, etc.) and **ἱκέτις** (Herotodus 4.165.3, etc.). The latter, in its earliest occurrence (**ἱκέτιν**, accusative; Aeschylus, *Sup.* 350, 428), can well be understood as 'suppliant's daughter,' since Danaus himself is a suppliant too. But elsewhere (e.g., Sophocles, *O.T.* 920) the woman is a suppliant in her own right. The secondary character of **-τις**, derived from **-της**, and its relative lateness are borne out by the retention of **τ** before **ι** instead of changing to **σ**,[92] and by the accent: whereas it is simply recessive in most of the **-της** nouns and therefore gets placed on the penult, the rule of recession does not operate before **-τῐς**; instead the accent is held to the penult to match the masculine.[93] 4.8

Some of the **-τις** nouns are occupational terms, including the earliest one recorded, **ἐργάτις** 'working woman' in a fragment of Archilochus (206 West = Hesychius and Eustathius 1329.36 [on *Il.* 23.775]).[94] Subsequent Greek commentators took it to signify 'prostitute,' by a sort of euphemism (cf. *meretrīx* in Latin, literally 'a wage-earner'). But since the poet pronounced it as an insult against Neobule, whose aristocratic father had broken off their engagement, the meaning 'workingman's daughter' is more apt, besides being literal. That would not have excluded a connotation of 'prostitute' anyhow, according to the class sentiments of the Greeks. 4.9

On the whole, when the sense was merely 'a woman doing such-and-such,' **-τρια** came to be favored over **-τις** and **-τρίς** (see above, §4.4). To be sure, the **προφῆτις** 'prophetess' at Delphi (Euripides, *Ion* 42, 321, 1322; Plato, *Phaedrus* 244a) was not an hereditary office. In some metaphorical way she may have been envisaged as the daughter of Apollo, **Διὸς προφήτης** 'Zeus's spokesman' (Aeschylus, *Eum.* 19), because he possessed her and made her communicate his messages to mankind. There was also a man at Delphi called **ὁ προφήτης** (Herodotus 8.36.2-37.1). His function certainly differed from that of the **προφῆτις**; whether or not they were ever of the same family, we are not told.[95] 4.10

But there is a set of terms expressing hereditary social status apart from occupation, and the feminine ends in **-τις** from a masculine in **-της** (vocative **-τᾰ**): 4.11

πολῖτις 'citizen's daughter' (Sophocles, *El.* 1227, etc.; **πολῖτης**, *Il.* 13.533, 15.339, a son of Priam, and as a common noun in 22.429, Euripides, *Bac.* 271, etc.);
δημότις 'townswoman' (Aristophanes, *Lys.* 333; **δημότης**, Plato, *Ap.* 33e, etc.);[96]
κωμῆτις 'neighbor' (Aristophanes, *Lys.* 5; **κωμήτης**, *Nubes* 965, etc.; **κώμη** being an urban district corresponding to a rural **δῆμος**).[97]
By analogy with **πολῖτης, πολῖτις** from **πόλις** 'citadel' and hence 'state,' as well as **Συβαρῖτης, Συβαρῖτις** from **Σύβαρις** in Italy (Aristophanes, *Ves.* 1427, 1435, 1438; Herodotus 6.127.1; etc.), **Ναυκρατῖτης** from **Ναύκρατις** in Egypt (Herodotus 2.97.2,179; Callimachus, *Ep.* 39 = *Anth. Pal.* 13.25; etc.), and **Μεμφῖτης, Μεμφῖτις** from **Μέμφις**, also in Egypt (Herodotus 2.2.5,99.2-4,112.1, etc.; *Memphītis*, Lucan

91. See *The Indo-European and Semitic Languages*, p. 114. Of course the identification was not so absolute or permanent that as long as Πελασγοί remained in the Greek vocabulary it designated precisely the same people as {pliští^ym} in Hebrew. Cf. our word *Dutch* with *deutsch* in modern German: originally they meant the same, but no longer; see my article, "Non-paradigmatic Forms: Suppletion or Preemption?" *Foundations of Language*, VIII (1972), 346-359.

92. Chantraine (above, note 79), p. 339.

93. Cf. how the accent of the nom. pl. in the "first declension" matches the masc. of the "second declension" whenever there is such a masc. counterpart—e.g., Κορίνθιαι, Κορίνθιοι (Euripides, *Medea* 213; Thucydides 1.13.2, etc.; in spite of **Κορινθιά**, 3.74.3)—whereas otherwise it goes according to the nom. sing.: ζημιᾶ, ζημίαι 'punishment(s)' (Aeschylus, *Pr.* 329; Thucydides 3.45.1), but προμνήστριᾰ, προμνήστριαι 'match-maker(s)' (Aristophanes, *Nubes* 41; Euripides, *Hipp.* 589; Plato, *Theat.* 149d).

94. The masc. ἐργάτης is applied by the same author to an ox (fr. 35 West = *Etymologicum Magnum* 530.28), but elsewhere it is a workingman and especially a farmer (Herodotus 4.109.1, etc.). The attestation of ληΐτις 'plunderer,' an epithet of Athene (dative ληΐτιδι, *Il.* 10.460), may be still older than ἐργάτις, but no *ληΐτης occurs.

95. Euripides has Medea's old nurse speak of her as δεσπότιν τ' ἐμήν 'my mistress' instead of the more usual δέσποιναν, which would have fit the meter just as well (17, cf. 49). Here the original or morphological sense of 'master's daughter' may linger in δεσπότις; but elsewhere it evidently does not (Sophocles, *Tr.* 407, *El.* 597).

96. Membership in an Attic δῆμος or 'township' was strictly hereditary, regardless of any actual change of residence to a different part of the country or even a colony abroad; v. Schoeffer in Pauly-Wissowa (above, note 18), V, 6-7, and B. Haussoullier in Daremberg-Saglio, *Dictionnaire des antiquités grecques et romaines*, II (Paris, 1892; repr. Graz, 1963), 84.

97. ἀγυιᾶτις (Pindar, *Pyth.* 11.1) means the same but in a mythical context, describing Semele as neighbor of the Olympian goddesses. ἡρωΐδων (11.7) 'of a hero's daughters'—i.e., Semele and the other daughters of Cadmus and Harmonia. Cf. ἐξ εὐγενίδων 'from daughters of good family' (Josephus, *Ant.* 7.3.3[70]).

4.136, Ovid, *A.A.* 3.393), the suffix **–ῑ́της** and its feminine **–ῖτις** spread to cities whose stem was not in **–ι–**, all of them outside of old Greece:

Ἄβδηρα, Ἀβδηρῑ́της (Herodotus 1.168, 8.120, **Ἀβδηρῖται**; etc.);

Ἄναια, Ἀναιῑ́της (Thucydides 3.19.2, 4.75.1, etc.);[98]

Μαρώνεια, Μαρωνείτης (accusative **Μαρώνειαν**, Herodotus 7.109.1; Xenophon, *An.* 7.3.16; etc.);

Ἱεροσόλυμα, Ἱεροσολυμῑ́της, Ἱεροσολυμῖτις (Matt. 2:1; Josephus, *Ant.* 20.8.5[163], *Bellum* 1.28.5[562]; etc.).

In all cases, so far as known, citizenship was normally transmitted from father to son or daughter.[99]

The Hebrew feminines ending in {–íʸt} are not occupational but only ethnic – apart from still other semantic categories that are irrelevant to our comparison with Greek (such as the ordinal numbers; e.g., הַשֵּׁנִית {haš·eníʸt} 'the second,' Gen. 4:19, etc.; masc. הַשֵּׁנִי {haš·eníʸ}, 2:13, etc.). Evidence of contact with the Greeks, direct or indirect, is very narrow in this part of the vocabulary. For neither society was much in the habit of categorizing women in terms analogous to those used in categorizing men, since a woman would pass her days mostly unseen or unnoticed (cf. Thucydides 2. 45.2). Still the Greek nouns of occupation that we have examined in this section do illuminate an economic development involving women, however modest it was in comparison with the multiplicity of men's skills. Furthermore, the ones in **–τρίς** and perhaps some in **–τις** show how several of the women's trades got started within the family, passing from father to daughter.[100] 4.12

98. **ναῦς . . .** Ἀναιῖτις 'an Anaeite ship' (8.61.2).

99. An amusing sidelight, which has affected the vocabulary of English and other modern languages, is the specialization of –ῖτις attached to νεφρ(ός) 'kidney,' πλευρ(ά) 'side,' and other parts of the body, with νόσος 'sickness' either expressed or understood: νεφρῖτις (Hippocrates, *Co. pr.* 30 [502]; νόσον νεφρῖτιν, Thucydides 7.15.1), πλευρῖτις (Aristophanes, *Eccl.* 417; Hippocrates, *Aph.* 3.23, etc.). The metaphorical use of the suffix was apt for *chronic* diseases, citizens (so to speak) of their respective organs.

100. A summary of this paper has been addressed *in absentia* to the VI Convegno Internazionale di Linguisti in Milan (September 1974).

It is a pleasure to acknowledge the invaluable help I have received from Lucille Urban and Binx Keefe in clipping and attaching all the footnotes and corrections, besides easing my other problems. I am also grateful to Herman Paikoff for photographing the Arabic words. All three are on the staff of the State University of New York at Binghamton.

V

Linguistics and Its Interfaces

A. LITERATURE AND FOLKLORE

POETRY AS SELF-CREATION

Roger W. Wescott
Drew University

Let us begin by defining psychotherapy as a remedial response to self-destructiveness, whether that destructiveness takes the blatant form of suicidal behavior or the attenuated form of neurosis. In these terms, the clinical aspect of psychotherapy becomes, primarily, the attempt to remedy psychopathy by reducing destructive behavior. In what we may call its poetic aspect, on the other hand, psychotherapy then becomes, primarily, the attempt to remedy psychopathy by augmenting constructive behavior. In practice, however, both procedures tend to yield the same therapeutic result; and ideally they are practiced in conjunction rather than so handled that one is employed to the exclusion of the other.

Poetry in action takes four forms: writing, reading, reciting, and listening to verse or poetic prose. While verse may be defined as metrical literature, poetry is harder to define. Etymologically, of course, it means "creation" and comes from Greek poiéō, "I make."[1] This view of poetry as construction, though no longer common, has not been wholly lost in our millennium. Of his deceased friend the writer Edward King, Milton, for example, wrote (in Lycidas)

> Himself . . . to build the lofty rhyme.[2] he knew

But art, whether literary or otherwise, is only one expression of man's creativity--a creativity that manifests itself in all aspects of his life. In 1936, British archeologist Gordon Childe wrote an anthropological classic entitled Man Makes Himself,[3] the thesis of which has since become axiomatic for most social scientists. All men, he said, are self-made in the sense that they themselves create the microenvironments which in turn mold all who live in them.

Mention of the term archeology inevitably raises the question of the evolution of human artifice, one component of which is poetry itself. In the broad sense in which poetry is equated with construction, man, of course, has never been without poetry; for man, alone among the primates, has been making tools of such an imperishable material as stone for at least a million years. In this sense alone man could well be defined as the long-incumbent poet laureate of the primate world.

If, however, we define poetry more narrowly as the artistic employment of rhythmically repetitive language, then it becomes much

more difficult to date the appearance of poetry. For the whole question of the origin of language is such a vexed one that, during the first half of this century, most linguists and anthropologists flatly refused to enter into any public discussion of it. But, after World War II, American ethnolinguists Charles Hockett[4] and Morris Swadesh[5] re-opened this long closed subject; and, bolstered by new evidence from paleontology and ethology, other scholars rather tentatively resumed their speculations on the topic.

Without going into technical detail about available evidence and the deductions that may be made from it, I reconstruct the development of those aspects of speech most relevant to poetry as follows:[6]

1. During the Paleoglossic Period (of pre-speech), about 1 million to 300,000 years ago, Pithecanthropians--of the type best represented by Peking Man--began to rival song-birds in their capacity for vocal imitation and repetition with variation. Though adult males probably utilized this skill primarily as a hunting-lure, women and children may well have practiced it as an esthetically rewarding form of vocal play.

2. During the Mesoglossic Period (of proto-speech), about 300,000 to 50,000 years ago, Neanderthalers of Mousterian Culture developed a productive form of vocal communication for social and religious purposes. With the syllable as its minimal unit, it probably sounded much like contemporary glossolalia, or "tongue-speaking." In its most ritualized form, it is likely to have been ideologically similar to prayer and acoustically similar to singing (as Otto Jespersen suggested in broaching his "Sing-Song" Theory of glossogony).[7]

3. During the Neoglossic Period (of "true" speech), about 50,000 to 5,000 years ago, Cro-Magnon Men of Aurignacian and Magdalenian Culture introduced role-terms and figurative language. These innovations probably brought with them such auditory art-forms as drama and poetry to accompany the visual art-forms of painting and sculpture which we know them to have created.

Speculation becomes unnecessary when we reach the historical era, about 3000 B.C. For written records show that the early Egyptians, Akkadians, and Hebrews had a poetic literature set off from their prose literature by a higher incidence of rhythmical regularity and lexical repetition. Not till the last millennium before Christ, however, did true verse--that is, poetic literature marked by strict meter and regular sound-repetition--appear in India, China and Greece.[8]

Returning now to Gordon Childe's theme of man as a self-created animal, we find that being self-created is far from the unmixed blessing that the traditional Emersonian stereotype of the

"self-made man" might lead us to believe. For man creates not only such boons as improved shelters and food-procuring techniques--those devices that have enabled him to dominate all other species on earth--but also such ills as war and psychopathy, which now threaten him with self-extermination. Yet true to the involute complexities of his nature, man further creates antidotes to the plagues he has created. Where there is demonic possession, there are shamans to control it; and where there is psychosis, there are psychiatrists to mitigate its rigors.

In its poetic or non-clinical form, the principle on which most psychotherapy operates is what Jack Leedy calls the "isoprinciple" of homeopathy.[9] This is the principle that drinkers invoke when, as a cure for hangover, they recommend "a hair of the dog that bit you." In a more scientific context, it is also the principle on which the medical practice of inoculation is based--that is, the principle that a controlled assault by pathogenic agents may strengthen the organism's resistance to uncontrolled assaults by the same agents.

This homeopathic principle, in turn, is closely related to the theory of catharsis developed by Aristotle in his Poetics,[10] in accordance with which one can effectively purge his destructive emotions--paradoxical as it may at first seem--only by accumulating more of these emotions till they reach a point of overflow and consequent extrusion. The best known modern application of cathartic therapy is, of course, Freudian psychoanalysis, with its insistence on the revivification and subsequent discharge of buried affect.[11]

No one, perhaps, has stated the principles of homeopathy and catharsis more succinctly and forcefully than the poet-engraver William Blake in his Proverbs of Hell, four of which follow:

> "The path of excess leads to the palace of wisdom."
>
> "If the fool would persist in his folly, he would become wise."
>
> "If others had not been foolish, we should be so."
>
> "Exuberance is beauty." [12]

Though the hazards of Blakean exegesis rival those of Biblical interpretation, I here presume, on the basis of a strong artistic identification with the poet, to recast his thoughts in a discursive, non-aphoristic form:

> We become fully aware of our feelings only when we let them spill over.
>
> It requires tenacity and courage to acknowledge a destructive emotion long enough to attain understanding and control of it.

Unless artists and scientists grapple openly with destructiveness in their writings, most of their readers will be unable to prevent their own destructiveness from erupting directly into action.

The free outpouring of emotion is not only therapeutically useful but also esthetically satisfying.

During the early 19th century, the Blakean outlook on art and life was, if anything, even better exemplified by the German than by the English Romantic poets. The healing powers of nature and of man's poetic response to it are well expressed in Annette von Droste-Huelshoff's composition "Parting by Sea":

Farewell, my love! It had to be.
   Set sail. Desert these coasts,
Fleeing my castle, leaving me
   Here in a house of ghosts.

Leave me to walk the stormy beach,
   Watching the sea-mists fly,
Or climb the sea-cliffs, drenched in spray,
   Spurred by the petrel's cry.

Abandoned, yes, but not distraught,
   Shaken but never crushed,
I keep a-light those poet's eyes
   Where stinging tears once rushed.

No bitter woman's plea, my soul,
   No hate, no lover's curse--
Not while the sea still wakes in me
   The miracle of verse.[13]

In all honesty, however, it would be a misrepresentation to cite the aforegoing poem, with its mood of courageous optimism, as typical of the German Romantic Movement. A more representative specimen is Nikolaus Lenau's defiantly pessimistic composition "Grim Solitude," which, nonetheless, is equally Blakean in its insistent self-expressiveness:

Wind is bleak and stone is cold:
   Small is the comfort here.
Yet a rose blooms. Can she not proffer
   Tenderness or cheer?

Alas, she cannot. Deaf to you
   She weeps but for herself,
Knowing that death will ravish her
   By violence or stealth . . .

Down the dark alleys of the world
Ruin awaits us all.
Joyless I live. The crown of thought
Weighs on me like a pall.[14]

Since so much of what poets have to say is unconventional and their mood seems so often to be bitter, it is sometimes tempting to dismiss them as purveyors of morbidity and turn for solace to cheerful horoscopists or popular columnists with a message of moral uplift. This temptation grows understandably stronger when poets treat the topic that appears to engross them to the point of obsession: that of death. For not only do poets dwell on the inevitability of death, but frequently they take what looks like a ghoulish pleasure in anticipating its imminence, as when Shakespeare's Hamlet refers to personal extinction as

a consummation
Devoutly to be wished.[15]

Poetic treatments of death, however, are rarely confined to a preoccupation with the destruction of life or the extinction of consciousness. On the contrary, the death-theme, if only by implication, enlarges life by expanding consciousness. For man is, so far as we know, the only thanatognostic animal--that is, the only animal with any awareness of death (as opposed to injury or isolation) and consequently any feeling, whether of fear or desire, for death.[16] In love-songs and battle-odes, therefore, poets express, for all their art, only the most broadly mammalian emotions of devotion and defiance; but in dirges and elegies, they express a uniquely human sensitivity to the poignant brevity of individual existence. While love and hate may be ignorant, the apprehension of death always bespeaks a tragic knowledge.

The confrontation of death, to be sure, is no guarantee of its acceptance. Yet only those who face death can fully embrace it. And, in all probability, it is only those who perceive death as an aspect of the life-process rather than a denial of it who can both face and embrace it. It is this realization, I think, that makes Whitman's many invocations of death refreshing instead of depressing. In the 14th Section of his "Memories of President Lincoln" (more popularly, if less correctly, known as "When Lilacs Last in the Dooryard Bloomed"), Whitman apostrophized mortality with the words

Come, lovely and soothing death,
Undulate round the world, serenely arriving, arriving . . .

Praised be the fathomless universe,
For life and joy . . . but praise! praise! praise!
For the sure-enwinding arms of cool-enfolding death.

Dark mother always gliding near with soft feet,
Have none chanted for thee a chant of fullest welcome?
Then I chant it for thee, I glorify thee above all . . .

Approach, strong deliveress,
When . . . thou hast taken them I joyously sing . . .
Lost in the loving floating ocean of thee,
Laved in the flood of thy bliss, O death.[17]

Here, instead of that "pornography of death" which British ethnologist Geoffrey Gorer sees as making modern mourning so uncathartic and sterile,[18] we encounter a lyricism of love--not decorously blind, but wide-eyed with rapture.

For all poets, whatever their formal religious beliefs, elegies are in some sense epithalamia and death is in some sense a reunion. Both wedding verses and funeral verses, therefore, are celebrations of immemorial rites of passage. And in both, regret for the old estate mingles with hope for the new. Death and rebirth, then, are inseparable--not merely in the Pollyanna sense that every death portends rebirth, but also in the sobering sense that each rebirth demands a death.

For the therapist, the moral of this poetic realization would seem to be that each of his charges is somehow chained to a pathological past and that only by an (inevitably somewhat painful) breaking of those chains can the psychological prisoner be freed. In the terms of our earlier discussion, each patient must--hopefully, with the therapist's help but, if necessary, without it--anticipate, effectuate, and celebrate the death of his own sickness before he can witness the rebirth of his own health.

Since health is wholeness, functionally as well as etymologically, we may reasonably ask how and in what sense poetry promotes wholeness. The answer, I think, is that poetry speaks, in the words of Tennyson's Ulysses, "with many voices."[19] It does not confine its appeal to one type of action or to one sensory channel alone. It is at once the transmission of a message and the expression of a sensitivity. And it appeals to the eye as well as to the ear--not merely in the literal sense that it may be written as well as declaimed, but in the imaginative sense that it activates Wordsworth's "inward eye"[20] and allows us, however paradoxically, to catch the vibrations of Keats' "unheard melodies."[21] Just as Wagnerian opera was a concrete Gesammtkunst of drama, dance, and scenic art,[22] so poetry is a more abstract synesthesis of music, painting, and impassioned speech. Its meters exhibit pitch, tempo, and volume; its imagery, color and form; and its discourse, a polysemic spectrum of concurrent messages, ranging from the highly explicit to the wholly subliminal.

The topic of subliminality, in turn, raises the perennially intriguing question of the relation between dreams and art-works --specifically, in this case, between dreams and poetry. Undoubtedly the relation is manifold. First of all, poetry and dreaming share a qualitative range far wider than that normally encountered in prose or waking life. Prose, like waking experience, tends to be confined, perceptually, to a constant but subdued polychrome and, emotionally, to an attitude of pragmatic realism; whereas poetry, like dreaming, tends to traverse the perceptual and affective spectrum from misty passionlessness to terrifying vividness.

Second, both poetry and dreams seem to have a compensatory effect on the deficiencies of prosaic and waking behavior. Fantasies unexpressed in prose and unacted in waking hours are freely voiced in poetry and acted out in dreams. Poems and dreams, then, provide an Aristotelian catharsis countervailing the no less Aristotelian moderation of prose and waking life. This countervalence, moreover, is therapeutic as well as esthetic and cognitive in nature. The unread or unwritten poem may erupt as an ulcer--a speculation that gains credence from A. E. Housman's observation, in The Name and Nature of Poetry, that good verses rise, like beer-fumes, from the stomach[23] (on which point he differs only marginally from Carlyle, who held that they were born in the small intestine!) [24] Either way, poetry may be regarded as a pressure-relieving outlet, quite comparable to the psychic safety-valve constituted, in Freud's view, by those hypnoid hallucinations that we call dreams.[25]

It would, to be sure, probably be more accurate to equate poems with day-dreams than with nocturnal dreams. For, as a result of the work of such physiologists as Nathaniel Kleitman, we now know that the dreams which punctuate our sleep are long in duration and narrative in form[26] and that these traits contrast with the brevity and imagistic quality of day-dreams. (The brief image-fluxes which often attend the transition from waking to sleeping seem to me to be more like day-dreams than like night-dreams. I term them reveries and characterize them as poem-like in form. Night-dreams I see rather as being novel-like.)[27] No doubt this distinction would have proved congenial to Poe, who protested, in The Poetic Principle, that "a long poem is... a contradiction in terms."[28] To him, epics and didactic verses were little more than disguised prose.

But if, like most critics, we accept epics and lyrics as equal in poetic status, we might, in psychotherapeutic terms, compare an epic with the step-by-step progress of analytic work and a lyric with the sudden flash of insight which relieves and facilitates analytic toil. So considered, epics and other long poems represent vital continuity, while lyrics and other short poems represent Maslovian "peak experiences."[29]

In any case, poems bear one important psychological similarity to dreams--namely, that they serve as richly useful analytic material for the therapist in his search for clues to the latent emotional currents in his patients. In some ways, in fact, poems are superior to dreams as sources of insight. For, while it is true that most poets edit their thought-stream before committing it to paper and, in the process, censor a considerable amount of the material unacceptable to conscience, it is equally true that most dreamers similarly (if more unconsciously) "edit" their dreams in the process of recalling and relating them. And poems have the advantage of being, for the most part, clearer and less fragmentary than dreams; so one can more readily return to them for discovery of new insights and verification of old ones.

Whatever view we take, moreover, on the subclassification of poems and dreams, the poetic experience and the dream experience seem to share a striking tendency toward ego-transcendence. This tendency is perhaps more obvious in the case of dreams. For nearly all of us have had the experience of dreaming that we were engaged in behavior quite unlike that which typifies our waking state, so that, for all practical purposes, we were "not ourselves" for the duration of the dream.

In the case of poetry, this same experience was apparently equally common among the ancients. In fact, pre-Christian poets, to judge by the language they used, often underwent, while composing, the kind of ego-loss that we associate with the phenomenon of spirit-possession among contemporary preliterates. In the Western world, trance-writing is regarded as an aberration, and it is generally felt that the few poets, like Coleridge, who composed in a state of unconsciousness, would not have done so but for their pathological dependence on drugs.[30]

The ancients felt differently about this. To them, poetic inspiration meant, quite literally, "in-breathing"--that is, the penetration of the poet's spirit by the spirit of another being. This "other being" was usually taken to be that of a deity. Among the Greeks, who developed one of the richest early poetic literatures, the goddesses who inspired artists were highly specialized deities called Muses. (The English word "muse" comes, through Latin mūsa, from Classical Greek mousa. The reconstructed original of this term is Pre-Mycenean *mont-ya, meaning "mind."[31] The idea apparently was that, in the process of artistic creation, the artist's mind was altered or supplanted by the mind of one or another of the supernaturals.) Though the term "muse" has continued to be used by European and American verse-writers, most of them now regard it as no more than a quaint historical survival or verbal convention.

If, however, I may speak autobiographically for a moment, I can vouch for the fact that--particularly during my adolescence--verse-writing long had for me a distinct aura of the uncanny.[32] When I wrote essays or reports, I knew that I was the author, just as, when I swept stairs or sidewalks, I knew that I was the sweeper. But when I wrote verse, I usually felt--with a mixture of uneasiness and excitement--that I was not the author but merely the amanuensis and that someone (or something) else was dictating the lines to me. Having a ghostly friend or invisible helper was a highly unsettling experience. For, while I was pleased to have my habitual solitude relieved by congenial company, I was distressed by my inability to discover who my visitor was; and, while I was delighted to appear to be the author of works which I was convinced were quite beyond my ability to compose, I felt vaguely guilty about taking credit for them and increasingly apprehensive that eventually I would be exposed (perhaps by the real "author") as a plagiarist! It was not until, at the age of 18, I began reading psychoanalytic literature that I lost my poetic anxiety and began to accept the idea that the verse which was "dictated" to me came, not from a disembodied person, but from my own unconscious mind. I then reassured myself that, if my unconscious belonged to me in the same sense that my hand or eye belonged to me, my poetry also and with equal right belonged to me; and eventually I felt self-absolved from the unspoken charge of literary theft.

Let us return now to our initial theme of poetry as self-creation. If, as the preceding paragraphs imply, poetry is generally not a product of the ego, we seem to be led rather to the view that poetry is a kind of self-obliteration than to the view that it is self-creation. I believe, however, that the apparent contradiction here can be resolved by making a distinction between the ego and the self. If we describe them as having the relation of part and whole (so that the ego is always contained in the self, but most of the self is not ego), the paradox disappears. The self is then seen as embracing all of the Freudian endopsychic entities--super-ego and id as well as ego--and perhaps some Meta-Freudian entities as well, such as Marcuse's super-id, or self-sublimated instinct.[33]

In this view, poetry does indeed create the self. It does so, however, not by generating such sub-selves as ego or id (for these are presumably pre-existent) but by integrating psychic entities which too often subsist in a state of mutual isolation and rejection. In its genesis, poetry is a spark that leaps from one sub-self to another, bridging the gap between them. In Jungian terms, it is a communicative link between the unconscious and collective on the one hand and the conscious and individual on the other.[34] And, if poetry has the power of integrative action within the individual, it is hardly surprising that it should also have this power between individuals, putting the poet into intimate contact not only with his

muse but with his audience. Nor is it surprising that poetry can continue to work its magic even in the absence of the poet. For the psychic ripples set in motion by the poet's pebble have no predetermined limit: there is no end of selves to be created. Through poetry (in the words of Tennyson's Bugle-Song)

> Our echoes roll
> From soul to soul
> And grow forever and forever.[35]

Furthermore, what poetry has done for individuals it can also do for nations; and this is equally true whether we construe poetry broadly to mean self-renewal or narrowly to mean the production and appreciation of verse.[36] As an example of what poetry can do to benefit a country, let us consider the case of Japan since World War II; and, as an example of how its absence can harm a country, the case of the United States during the same period.[37]

If we begin by equating poetry with verse, the contrast is more obvious. For the reading, writing, and recitation of poetry have been a living part of the Japanese tradition since the 7th century A.D. Even today, there are few educated Japanese who cannot recite, or do a calligraphic rendering of, the more famous haikus of the 17th century lyricist Basho. For epigrammatic compression and imagistic appeal, it is hard--in any language--to match his "Yama-ji Kite":

> Mountain travel:
> Sudden beauty--
> Violet flowers![38]

The nearest English equivalent to Basho's haikus are probably the Songs from Shakespeare's Comedies. Yet few are the Americans who can recite them, and fewer still are those who consider verse of any kind worthy of serious attention outside a young ladies' finishing school.

Even in its broadest sense, moreover, poetry is far more in evidence in Japan than in America. While we have geared our economy to the suicidal art of war, the Japanese have re-tooled theirs to the art of peace. While we are allowing our population to expand without hindrance, they have succeeded in reducing their reproduction-rate to the point at which population-replacement is achieved but not exceeded. And while we continue to deplete our resources and pollute our environment, they have developed a system of ecological re-cycling whereby nearly all biological and industrial wastes can be productively transformed and so utilized as to maintain or increase their natural resources.

By this comparison, needless to say, I do not mean to imply that the Japanese are naturally more sensitive or more constructive than we Americans. In large part, it has been the constrictiveness of their habitat that has pushed them into compressing both their art-forms and their industrial enterprises. But the fact remains that the Japanese have exhibited what Toynbee calls a "creative response" to the perilous challenge presented to them by penurious Nature.[39] Because Nature has been more bounteous to America during its centuries of Europeanization, we have been able wishfully to postpone our inevitable day of collective reckoning. At present rates of growth in population, transportation, and industry, however, our country will soon be, in effect, even smaller than Japan now is. Yet it is now, when we can cearly envision without having fully to experience this plight, that we are in the best position to take remedial steps: to practice a polysemically poetic therapy ourselves. When and if we do, we should be able--like Whitman and his many overseas auditors--once again to "hear America singing."[40]

Afterword: In the 1960's, when the topic of this article was first conceived, Japan was regarded as being, ecologically speaking, among the world's most progressive nations. By the early 1970's, however, her productivity had so far outrun her precautionary measures as to make the Tokyo area one of the world's most polluted metropolitan regions. It remains to be seen whether a combination of national prudence and oil scarcity will bring about yet another swing of Japan's ecological pendulum.

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LEGEND AND MASS MEDIA*

Linda Dégh and Andrew Vázsonyi
Indiana University at Bloomington

The legend is polyphonic. While the leading voice sounds, the common knowledge, the common culture of the narrator and the other participants of the event resound in the legend, as a pedal point, more emphatically than in most other forms of communication. (This is why, contrary to the verbosity of the tale, the legend is succinct (Bausinger, 1968. pp. 180-81). There is no need and no way to communicate the precedents, the underlying beliefs and other particulars in each case.) Legends are interrelated; whoever recites one, takes the others for known and feels that to hint at them is sufficient (Dégh, 1965. p. 83). The legend-teller can depend on community knowledge of a particular belief. Moreover, "Both the performers and the audience have to be in the same situation and be a part of the same reference group" (Ben-Amos, 1972. p.12). This situation was described in more detail by Honko (1962. pp. 96-98) and more recently by a semantical analyst — in not quite the reticence of the legend — as follows:

> The ethnocultural group has at its disposal a *belief-language* which means that certain members of the belief-vocabulary, the list of relevancies, together compose meaningful communications, naturally when the gaps between the relevant composite parts are filled in with the appropriate linguistic bonding agents."
> (Hoppál, 1969. p. 413).

The common knowledge about the Devil necessary for the understanding of many folk legends (Rohrich, 1965. pp. 28-58 and 1971, pp. 21-35), for example, seems to be curiously dissimilar in Europe and in America. In the course of our experimentations[1] we used, among others, the text of an old German folk legend that was already the subject of serial reproduction in the famous test of Walter Anderson (Anderson, 1951). We have read this legend to a sequence of several young and middle-aged Americans. As the story goes, at the turning point, the passive hero finds out that the beautiful seductress has horse's hooves instead of normal human feet, and he flees in fear and desperation. For the European recipient, whose general belief has strong roots in medieval demonology, it is quite evident and does not need further clarification that the lady had hooves; that is, she was the Devil himself. No wonder the young man in the story is so scared. Nevertheless, most of the young and some of the older Americans interpreted this key incident of the legend in a different way: the lady had a deformity, a misshaped, twisted foot, or "clubfoot," understandably repulsive for a young man. The rational interpretation, however deprived the legend of its original demoniac connotation and rendered the despondent flight of the man with the evil spirit in pursuit senseless, or even comical. Further discussions with the interviewees, especially with the

* This paper is part of a larger essay to be published in World Anthropology: Papers and Conference publications of the IXth International Congress of Anthropological and Ethnological Sciences, Folklore in the Modern World, Vol. II, Mouton Publishers, The Hague, 1975.

young people, revealed that with a few exceptions they did not know that the Devil had hooves.

If the European common knowledge about the Devil differs so considerably that Americans cannot enjoy the thrill of this legend, how about another traditional legend character — the witch? The answer came readily and without hesitation from many of our young informants. The witch seemed to be a very popular person. Her outward appearance, as depicted by the majority, followed that of the heroine of the popular TV serial "Bewitched," and her nature, the paraphernalia surrounding her was also that of the lovely Samantha. The pretty housewife became the epitome of the "witch" of contemporary American lore and the model of the ambitious local and nationally prominent witches, with an "official witch" in the lead.[2] Evidently, this witch-concept strongly deviates from the traditional and still commonly known European witch that is otherwise not unknown in America, either.

In a few years the television series accomplished what ongoing oral tradition could not have done in centuries. It established and perpetuated a profound common knowledge in many respects deviant from tradition.

Interaction of folklore and mass media had been underestimated even by those who are well aware of its significance. The so-called *Zeitungssage* (newspaper legend) did not escape the attention of folklorists; its impact on oral tradition was already noted some fifty years ago (Wesselski, 1931, 1935; Anderson, 1960; Peuckert, 1965. p. 38). That the radio is able to stimulate instant legend-formation became most remarkably known to the world when Orson Welles presented his radio drama *War of the Worlds* shortly before the outbreak of World War II. It is well known that "newspapers live from legends," especially in the "dull summer season" (Peuckert, *ibid.*; Bausinger, 1968. p. 219), and it is also known that erroneous or, for some reason, distorted communications of newspapers, radio and television stations often have a great impact on the folk legend. In such cases the legend often takes over the distortions of the mass media.

Here is an example, the most current among all we have heard lately. In the state of Texas and also beyond its limits, curiosity-seekers, but also special investigators, are exploring the nature of a pulsating, bleeding, "cancerous blob composed of matter which mysteriously oozed from the ground" in the Dallas area. The account was later enriched by new elements, namely, by linking the phenomenon to a "space ship" that crashed and burned in 1897 at Aurora, 75 miles to the northwest. It was also reported that the spaceman, arriving from a remote planet, buried in the Aurora graveyard by the villagers, might have come to a mysterious new organic life in the shape of the blob, pulsating like a heart. According to the Texans — and this is highly probable — the two legends are in no way related. Like folk legends in general, these also have some feasible base: a spreading fungus bleeding purplish-red material and a grave in the cemetery long reputed that of an extraterrestrial creature. In spite of the considerable distance between the two, *The Austin American* (May 29, 1973) connected the two legends with each other, probably under the influence of space flights and science fiction. This way, the planet-man legend that bloomed at the turn of the century and "cooled off" decades ago was reactivated and is now progressing in its new form. What will be its fate? Will it live in its new, more complex form, will one or another

part of it waste away, or will it disappear completely? It is too early to tell.

In countless cases the newspaper, radio, or television was instrumental in the creation of a new legend, as well as in the revitalization, modification, dissemination, and maintenance of an old legend (Weisser, 1968. pp. 401-14). The mass media are also responsible for the weakening, disintegration, and vanishing of a legend, by publishing negative legends, disclaiming legends, or introducing other, more up-to-date legends. Following the instructions (or the misinterpretation of instructions) of mass media reports, one by one valiant men armed with their shotguns invade vast forests or climb dangerous cliffs to save the world single-handedly from ferocious monsters; amateur frog-men submerge in seas and lakes to meet personally all sorts of giant sea creatures.[3] Masses assemble in front of buildings all over the country where day after day, at a certain hour, an immaterial pale human face appears.[4] How many times have newspapers had to revoke their well publicized legend about snakes in imported sweaters (Mullen, 1970. pp. 214-28) from fear of bringing on a lawsuit by dry-goods store operators? And how many times have police been summoned to disperse cars causing a traffic jam because — as announced by the daily paper a few hours earlier — friendly outer-space mannikins landed on the nearby meadow to observe life on earth, comfortably leaning against their spaceship?[5] Just recently, eleven ghost sites were spotted in Chicago by a newspaper reporter. *Chicago Daily News* (April 7-8, 1973) published the pertient legends accompanied by a map with the location of the haunts. To sum up: there is no doubt whatsoever that mass media draws heavily from folklore, and vice versa.

Nevertheless, this conclusion does not really differ from what could be said about literature long ago. It is certain, for example, that it was decisive for the fate of the legends incorporated in the *Deutsche Sagen* that the Grimm brothers had published them. These legends were absorbed again by the folk in the course of subsequent decades, so that after more decades had passed the legends could again mount the stairs to literature, and so on and so forth, repetitively in slow motion, but continuously following the rules of tradition. If we wanted to sketch graphically this process we would draw a long, mildly curved wavy line that fluctuates slowly and comfortably between the two levels of folk tradition and literature, encompassing great epochs with each curve.

The diagram of the relationship between modern mass media and the orally transmitted folk legend would look entirely different. The chart would consist of restlessly fluttering dense and steep straight lines converging in a sharp angle representative of the connection between the legend which is passed on orally and its non-folkloric elaboration. Let us assume that something happens Monday morning that could become the core of a legend. As it happens, a reader calls the editor of the local paper and communicates the message, whereupon the afternoon edition will already carry the novelty. The same evening, everyone in town talks about it. If the novelty — that often might be considered rightfully a folk legend — is interesting enough, the local radio station might also mention it in a few words and some pictures taken on the scene might also be shown on television. Indeed, there is no reason why the story could not enter the same day the national or even international stream of the legend process. By nightfall another reporter might encounter the story, already on the oral tradition level, and transmit it

further to the news desk of some communications medium. Eventually, on Tuesday morning, the public might be introduced to a new version of the legend. This lightning-fast process cannot be perceived authentically with any kind of human sense. Who can possibly know where a legend had started out, what its original form was and what road it followed? Who knows how much the mass media contributed to it and how much the genuine oral folk tradition, in its variation, degeneration and interlacing with other legends supplied? And who could state with good reason that, let us say, the TV announcer (one of the "folk" after all), who had heard the story in question just a little while ago and communicated the new folk legend verbally, more or less improvised is not only a participant of folk *tradition* (like the contributors of other mass media) but also of *oral* folk tradition?[6]

The quantity — the number of repetitions of reciprocal effects — had been transformed into quality. The relationship between the Grimm Brothers and the folk is far from being identical with that of mass media and the folk. The creation of the Grimms can be easily distinguished from orally transmitted folklore, while the components of the relationship between "mass media and folk" practically cannot be, and theoretically would not even be, necessary to separate one from another. One could hardly find contemporary legends that had not traveled by means of the intricate labyrinths consisting of diverse transmission media: immediate, face-to-face oral communication, letter writing, telephone message, humorous and serious newspaper report, radio and television information, cartoons and comics, popular and perhaps also scientific publications. Where are we from the time when Bascom's categorical opinion that "all folklore is orally transmitted" (Bascom, 1953. P. 285) could have been considered acceptable! Pentikainen already acknowledged the necessity of the study of booklore and mass lore, the consideration of the partially or entirely non-verbal materials by the folklorist (Pentikainen, 1970. pp. 92-93). Nowadays, however, it would be pedantic to try and determine which vehicle carried the legend, just as if someone would state that he traveled from the Bronx to Long Island "partly by subway and partly afoot" just because he had to walk a few steps when changing trains. It is not enough to acknowledge that mass media has a "role" in modern legend-transmission. It is closer to the truth to state that the mass media are *part* of folklore — maybe the greater part. The legend mades a part of its way — presumably the lesser — on foot and continues on the longer trail through the speedy modern vehicle.

At the given speed the concept of "common knowledge" has immensely widened. The elementary notions of the legend — the theses of folk belief upon which certain legends were founded at a given time and place — belonged earlier to a much smaller group of people than nowadays, when the jovial demonology of "Bewitched," the vampire-ritual of Dracula, the conventions of wolfmanhood entered the notion-inventory of those who turned on the TV or forgot to turn it off. This condition also necessarily caused a new division: the polarization of "readers and the nonreaders, the listeners and the non-listeners" (Riesman, 1965. pp. 419-20). The truth is, however, that common knowledge of the ingredients of the legend more or less encompasses everyone. The pedal point of the legend sounds deep, strong and steady in almost all society. Yet, he who listens closely will be able to discern other constant voices in the intricate, lively polyphony of the folk legend. Following the traditional rules of the counterpoint, these voices depend on each other but at the same time are also independent from each other.

The legend-narrator has to reckon with the familiarity of a certain *Glaubensvorstellung*. However, this in itself presupposes a large amount of precursory knowledge. The legend is addressed to those whose common knowledge is already taken into account, and thus it does not need to explain much. The listener is either in the clear about the difference between "hoof" and "club-foot" or he does not understand the story of the horse-footed devil. Evidently, many other kinds of preliminary notions are also necessary for understanding the legend and especially for comprehending its connotations. For example, the receiver of this legend has to know about the social life, mores and public spirit of an old German small town. He must be familiar with items in the setting: the carriage and pair, the inside and outside of the three-story palace of a nobleman, the nobleman himself and his servants, the promenade, the charming lady (whose long skirt, fashionable at the early part of the nineteenth century, could cover the Devil's hooves), the sound of the flute, the melody of the exorcising chant and, above all, the intent and the nature of the Devil and how to handle him — just to mention the most obvious from all the indispensable notions. These cannot be learned from school books or from life experiences but mostly from other legends that support and complement each other.

What is a real witch like? Legend-tellers and audiences in places where old-style witch legends prevail know her well. Those who model the features of the witch after televisions's Samantha will not be able to cope with the traditional witch, the evil old hag who spends her nights carousing with the Devil and whose greatest concern is to dry the cows of the neighbors, cast evil spells, turn herself into a cat and cause people to have nightmares by sitting on their chests before dawn.

Were the necessary notions of the legend ever, or are they now, common in certain localities? If we collate the number of contemporary participants — the adepts of legend notions — in the legend-process, with the total number of people in society, it will become clear that the ratio of general legend notions was smaller in the past than today. This is understandable, taking into account that for the most part those who attended verbal legend-telling occasions were to some degree, regardless of their attitude, interested in hearing legends. Supposedly, these people had an almost thorough knowledge of legend notions, what they did not know yet could be learned right there on the spot. On the other hand, those members of society who did not participate in legend-telling events could obtain information at best only indirectly and in much less detail. Consequently, only a part of society was educated in this area of knowledge. (This also concerns those countries where presently mass media are not as developed as in the U.S. and other Western countries.) The assumption of folklorists that the person *well versed* in legend notions was in many cases identical with the *Believer* probably caused the illusion shared by many theoretical authors that not only knowledge but also acknowledgement of the *Glaubensvorstellung* is a postulate of the legend event. Thus the conclusion drawn from field experience in old-fashioned European peasant communities — that belief in the legend statement belongs to the definition of the legend — is in need of revision.

If nowhere else, conditions in the United States will make this clear. A young folklorist in the spring of 1969 performed a statistical assessment of the folklore materials in a day's program of a television station (Burns, 1969. pp. 90-106). Among others he found no less than twenty-four items — among them

children's cartoons, situation comedies and advertisements — that were based on folk belief. In front of the TV screen the natural selection of legend-telling occasions, evidently, can be affected only with limitations. That is to say: only those who feel like it take part in the events. Television, this precious device, occupies the central spot of the living quarters "formerly reserved for the house altar" (Riedel, 1965. p. 27). Regardless of what motivates a person to turn on the TV set or cross a room in which it is on, he will frequently obtain instruction in basic legend notions. Magic characters, the supernatural and wondrous objects invade the viewer's daily life: a wizard pops out from the wall with the best floor wax one could desire; a bottle bursts and its captive spirit produces a new detergent; a radiant heavenly messenger carries perfectly sealing plastic bags; spots are cleaned by magic in front of the viewer's eyes; margarine can transform the consumer into a king; and a brand of hairspray puts even a witch's magic wand to shame. On the ominous side, there are of course the demons, warlocks, monsters and ghosts. The memory of the viewer inevitably retains all these characteristic features.

However, TV is only one of the many vehicles through which the legend penetrates everyday reality. In addition to TV, radio and movies, there are daily papers and magazines, comic strips, children's books, toys and outfits: paper and plastic monster masks and costumes are sold to all consumer markets. The average housewife finds this sort of educational material in food items for children, as for example, the cereals "Count Chocula" and Franken Berry" with "inside monster erasers" and a "monster towel." It is as if all the media were enthusiastically working on the intermingling and perpetuation of potential legend nuclei — the element of belief concepts — in daily life.

What is the effect of this forceful, insistent indoctrination on people? We did not subject this question to any serious scholarly scrutinization and we did not know anyone who studied it from the viewpoint of folkloristics. We are compelled in this matter to resort to guessing. Presumably, the legend apprentice, in spite of himself, to begin with will learn that vampires have two long fangs, that the person whose hands suddenly begin to turn hairy is about to be transformed into a wolfman, that the white sheet (as everyone has always guessed) is the costume of the dead emerging from its grave, that Frankenstein's monster limps a little bit and that the Mummy cannot be harmed by gunshot. In sum, the would-be legend-bearer will easily learn the entire body of legend-propaedeutics without difficulty. But first, he has to examine himself to determine if (using vague terms) he is in favor of this remythicized universe or bored by it, if he is chilled enough to shiver or just annoyed at the encounter. He then is obviously forced to take this position: does he believe or not? If he believes, to what extent and how? Does he accept some of the belief concepts, the majority, all or none of them?

Still, why do we view this as a modern phenomenon? Why do we relate it to mass media? Legend-tellers and listeners, as we have suggested earlier, always took a stand in connection with the position of the legend and seldom concealed their opinions from the participants of the legend-telling session. The legend is the projection of folk opinions; hence by definition it provokes the expression of viewpoints. However, prior to the qualitative change brought about by the mass media, the number of legend-telling events were limited. Their participants, again using vague terms, were legend enthusiasts — active believers, unbelievers, hesitants, and others — whose numbers in relation to

the whole of society could not have been great. Normal life did not demand from the greater part of society to decide daily, maybe even more than once, unintentionally and more or less unaware, about the acceptance or rejection of a concrete belief concept. Nowadays the latter is encountered via mass media "feeding on" legends. It is as if nonpolitical citizens, who according to their choice were liberals or conservatives and expressed their adherence only by casting their votes accordingly once every four years, would have to visit the polling station every day to express their party-spirited opinion about unimportant matters. For most people this would be very cumbersome; nevertheless, there would be some, no doubt, who would find pleasure in such ritual. Either way, people would gain through familiarity with the details of public affairs. If the newspaper-reader learns from the morning paper that Resurrection Mary again rose from her grave, danced all night in the Willbrook Ballroom and then thumbed her way back to Resurrection Cemetery, he does not have to reflect very much. He remembers well from earlier press reports the sweet sad story of the girl eternally waiting for her dancing partner. He does not have to form an opinion about it; he already has one. However, should the reader not yet know about Mary, he certainly would be familiar with the composite motifs featuring Mary's fragile figure in the white dancing dress, and he would have also decided, as much as he could, how to feel about her story.

If, then, the common knowledge of legend notions becomes literally more and more *common* and extends to the whole society instead of remaining within a relatively small group, responses to legend statements will also assume social proportions. Proponents in the past could find people who did not know the actual story, or one slightly similar to it; consequently, their reactions were not immediate and final. The proponent today, on the other hand, most probably can count on an unanimous common opinion that has already formalized its attitude toward the legend in question or others similar to it. Legend-telling therefore is more apt to provoke dispute than ever before, to challenge oppositions, and to ask questions that do not expect ultimate solutions. The answer it receives is also provocative and invites a dispute that cannot end with an agreement. The contrapuntal unity of question, answer, statement, denial, doubt, certainty, belief and disbelief amount to the legend event, that is, regarding its social function: the legend itself.

Footnotes

[1]Laboratory experiments in multi-conduit transmission in the process of folk narrative communication are in progress under the sponsorship of the Research Center for the Language Sciences at Indiana University.

[2]"The Good Witch of the West" (*Life*, April 10, 1970) who "casts only kindly spells" and who has similes on local level (*Bloomington Herald-Telephone*, July 11, 1971) is certainly in the focus of popular interest. The article about her was commented on by two fourteen-year old girls, both "aspirant witches." One of them in her letter to the Editor wanted to find out whether one has to be born a witch or can one learn the tricks? "Some of the things I am interested in are casting spells and incantation powers. Could you help me to become a witch?" (*Life*, May 1, 1970). The other girl seems to be more knowledgeable. She "studied divination, incantations and such-like for only a year" and is very critical of the official witch: "I regret to say that the witch you describe...struck me as opinionated and ignorant" (*Life*, May 20, 1970).

[3]There is no end to the list of monsters reported by the media. Millions follow the exploits of "Nessie" the long-necked beast in Scotland's Loch Ness Lake and "Bigfoot," the "Sasquatch" treated by best sellers (Green, 1970; Napier, 1973) is also continually covered by the news. There is a pseudo-documentary film now showing, "Legend of Boggy Creek" about a swamp monster in the tiny community of Fauk, Arkansas. As John Wm. Johnson, graduate student in folklore, writes me, "a friend here on campus (University of Texas at Austin) who is from Arkansas, confirms the existence of the belief-legend." A modest local example is the 600-pound and seven-foot tall hairy, moldy monster of Monroe, Michigan, that grabbed a Volkswagen and gave a black eye to the driver, a 17-year old girl. After this, even monster hunters combed through the nearby forest for the beast. "Sheriff's deputies estimated about 1,000 persons, many of them armed with shotguns, rifles, bows and arrows, rope, flashlights and butterfly nets..." The hunt went on for months according to UPI reports in 1965 whilst "the monster was a boon to merchants in the area. Business was brisk at restaurants, service stations and motels. One drive-in is still serving monster-burgers" (*Bloomington Herald-Telephone*, August 17, 1965).

[4]"Face on Courthouse Window Haunts Town for 92 Years" writes *National Enquirer* (September 20, 1970). This is the tourist attraction of Carrollton, a west Alabama village. A pamphlet tells the story of a tragic lynching of an innocently accused man.

[5]Just one of the many books that attest to the truth of visits from outer space achieved nine printings in only the first half of 1973. The book by Erich von Daniken, *Gods from Outer Space*, would alone deserve to be noted as mass medium.

[6]Ben-Amos (1971. p. 4) notes that the folk musician today can perform for millions on a TV network "in a style and manner that approximate his own singing and playing in the midst of his own small group."

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V

Linguistics and Its Interfaces

B. PERCEPTION, KINESICS, AND TRANSMODAL CODES

INFORMATION, NONINFORMATION, ZERO-INFORMATION, AND CONTENT

Laurence Urdang

A boy throws a stone through a church window, clipping out the "E" from the word "HIGHEST." The congregation is now faced with the exultant theological call to give "GLORY TO GOD IN THE HIGH ST."

Please listen to this tape recording

(Recording is played.)

If you knew the title of the popular song I just played before today - that is, if you were familiar with the arrangement by this particular group - you will please not inform your neighbor of it.

You are driving down an unfamiliar street at night and a neon sign in a storefront window catches your eye:

BA & G LE

By the spacing, you can easily tell that there are letters missing and where they should be:

BA- & G---LE

As we all know, when all the letters are lit up, the sign reads "BAR & GRILLE." But this is intuited information, and it might be interesting to examine what other possibilities afford themselves to a solution. Unlike the open squares of an incomplete crossword puzzle or other word puzzle - like "Hangman" - in which the definition, however deliberately ambiguous it may be, provides a clue to the completed information, the only clues in most situations of incomplete information must be drawn from experience and especially from what I shall call <u>contextual experience</u>. Contextual experience improves with time, in a normal person's development, and is associated with his ability to assimilate familiar clichés.

In the instance cited, the following English words present themselves as possibilities for filling in the blanks:

BAA BAD BAG BAH BAM BAN BAP BAR BAT BAW BAY

GABBLE GAGGLE GARBLE GARGLE GENTLE GIGGLE GIRDLE

GOBBLE GOGGLE GUDDLE GUGGLE GURGLE GUTTLE GUZZLE

The opportunities that avail themselves for amusement - BAT & GIGGLE, BAG & GIRDLE? BAD & GENTLE - are here limited only by the restrictions of the initial G of the second word. In fact, some of the possibilities are reminiscent of British pub names; certainly, BAG & GIRDLE could be a not-too-unusual name for a shop selling handbags and lingerie. Yet, there are in contextual experience too few instances either of such a name or of such a shop's requiring a neon sign. Since there are 11 possibilities for the first word and 16 for the second, the number of combinations totals 176. If we add to these the onomastic possibilities (BRY & GREELE, BAX & GESSLE - almost anything imaginable), the number of possibilities soars.

But possibilities are only interesting when considered within the realm of

probability. For example, anyone with the slightest experience with English knows that the probability of a word's containing a sequence like BX, BQ, BZ, BT, and so on is quite remote. Also, in the example given, we restricted the ending of the second item to -LE; had the initial G been missing as well as the other letters, the possibilities in English would have increased to 602 for six-letter words ending in -LE, bringing the possible combinations to 6622. And the combination could only have produced BRA & GIRDLE had the A been missing from the first word.

These examples are conjectural but possible, and they have been drawn to illustrate some of the points I wish to raise.

I should like, for a few moments, to dwell on the idea of "sense." What do we mean when we say that something makes "sense"? To venture a speculative notion, I'd suggest that we mean something like 'prove meaningful within a given context; provide congruous information.' "Congruous information" has a less than obvious meaning unless contrasted with "incongruous information," so it might be useful to explain what is meant by congruous and incongruous in the context given.

In the normal course of events - congruous events, that is - when we are introduced to a stranger there are a limited number and kind of expressions that are customarily exchanged:

(I'm) pleased to meet you (and many variations, like Pleased to make your acquaintance, Nice to meet you, etc.)
Hi!, Howdy!, etc.
I think we('ve) met before (and variations, like Haven't we met (before)?)
I've heard of you! (with appropriate suprasegmentals)
I think I know your mother (father, brother, etc.)
Of course we know each other! (and variations)

While the observation might be true that such salutations are extremely numerous, they are not numberless, and it must be evident that they are somewhat restricted by the context in which they occur. And, talking about numberlessness, I should like to digress for a moment to destroy, once and for all, an illusion that has victimized many linguists for too long: the number of expressions, sentences, phrases - call them what you will - in English, in any given language, or, for that matter, in the sum of all the languages of the world (including all dialects, all idiolects, and all dead, moribund, and other languages) is not infinite, contrary to the schools of linguistics that assert them to be so. If one examines a language - let's take English - mathematically, it will soon be observed that there is a finite lexicon. That lexicon may be large - perhaps one million types - but it is finite. Even if all the tokens are included, it is still large - but it is infinite. Now let us assume outlandish bounds for the length of an utterance - say, 5000 words - and let us further assume that there is no syntax - that is, word order is not a restriction and all words can appear randomly throughout an utterance. The total number of permutations and combinations possible can be expressed as 5,000,000! (read as "factorial five million"). Admittedly, this is quite a large number, but people must learn to distinguish between "very large" and "infinite" - they are definitely not the same thing. It is possible, for example, to count the number of grains of sand on the earth or the number of molecules on the earth, in the solar system, even in our Galaxy; it has been calculated that the local group of galaxies - the 20-odd associated with the Andromeda Galaxy and the Milky Way - have a total mass equivalent to that of 500 billion suns. That, too, is a large number, especially when converted to grams, molecules, atoms, or electrons. But it is not infinite. Surely the number of combinations and permutations in a given language, under the constraints of grammar and syntax, can easily be shown to be far less than infinite.

What is really interesting about language, I think, is not so much that it

affords a very large number of opportunities for expression but that such an enormous amount of what is actually said by people is an exact duplication of what others have said uncounted times before and will probably repeat uncounted times in the future. By the time we have reached 20 or 30, or 40, it is a wonder that we haven't all died of boredom!

Returning to our somewhat simplified example of salutations, the point I wished to make was just this: given a specific context, the expectancy of the cliché one will encounter is confined to a rather limited variability. Most people probably don't even listen to what others say when being introduced. For instance, if, instead of using one of the accepted cliches when being introduced to a stranger, you were to say something like, "How do you like my socks?" or "I'll bet you're wearing green underwear," it is very unlikely that the other person will even hear you: he is likely to "hear" what he expected to hear; even if he does actually listen to you, he'll either think that you are insane or, more likely, that he hasn't heard you right.

Thus, I maintain that the congruity of the situation - the context - places demands on communication and comprehension of information that it fall within our normal expectations and is, henece, more or less predictable. An expression like "I knew you were going to say that!" would tend to belie all of the infinitude attributed to language, yet we say it and hear it all the time, especially from people who are familiar with - or who think they are familiar with - our thought patterns. If Benjamin Lee Whorf was right, then the basic thought patterns of the speakers of the same language cannot differ as much, say, as those of speakers of different languages, particularly of languages that exhibit little or no common structure. Thus, again, what is interesting is not that there are so many divers utterances - even considering unrelated languages - but that speakers of other languages are able to speak and communicate at all.

To put it differently, since we know that the structures of, say, Chinese, Arabic, Kwakiutl, and English are about as disparate as they can be, isn't it astonishing not only that one human brain is capable of speaking Chinese while another can speak Arabic but that it is possible to be bilingual or even tri- or quadrilingual among such widely divagant structures?

Those familiar with acoustic will know the term "white noise," a convenient one for discussion of another aspect of communication. White noise is sometimes called "background noise" and is simply noise that is there and that is heard but that is usually below our threshold of concentration. It is the sound of crickets, of birds, of the wind in the trees to to country-dweller; it is the sound of traffic, of people walking with hard heels on the pavement, of distant fire sirens to the city-dweller. At bottom, it is really any sound pattern, regular or random, that we hear, are accustomed to, but pay no conscious attention.

Much of language consists of white noise. If a natural utterance or section of a natural utterance is examined carefully, it is likely that only a small percentage of it will be found to contain any information: the "essential" information - really, the information - will be found couched in a mass of white noise.

It isn't exactly clear why this is so, but it may be because, remarkable thought it may be in its ability to process metaphoric symbology at all, the brain may be incapable of processing what I shall call "pure information" in any appreciable quantities. An example of pure information would be a mathematical or chemical formula. Neither of these contains any white noise at all if reduced to its simplest form. On the other hand, neither is ever used as a natural utterance, either.

Perhaps an analogy is in order here. Computers are capable of processing

information only in batches of certain size and configuration. Since the magnetic tape transports used for feeding data into these machines operate continuously, there is usually a device installed between the tape drive and the main frame of the computer, and this device acts as a buffer. In fact, it is a buffer. It is also called a buffer. Its function is to store data temporarily and feed it to the computer in digestible doses.

Isn't it possible that much of language and communication works the same way? That the white noise in language - the verbiage, the grammatical and semantic redundancies - is essential to our capacity to assimilate information? Surely, English spelling contains a great deal of redundancy. But what of the languages that have what is called "phonemic" spelling? Grammar offers many examples of redundancy: why should we have to say go with I, you, we, and they but goes with he, she, it? Worse, why should we have to say am, are, and is when only one would suffice - as many dialect speakers have found? To be sure, there are historical reasons for the forms, but we are still using them now, more than a thousand years after they were no longer necessary to convey grammatical relationships.

If I may return, for a moment, to the matter of boredom, it becomes less astonishing that we haven't all died from it by the age of 40 if you accept the premise that much of what we hear and read is white noise - in other words, we don't hear it or read it at all, for it becomes just so much traffic noise, bird-twittering, and what-have-you.

This suggests two possible points of departure. First, what happens when the white noise becomes too loud? The answer is that it interferes with the information. Second, I should like to offer the notion that if white noise when repeated remains white noise, then it must be repeated information that creates tedium and boredom.

Other, familiar examples of white noise are such speech habits as the perpetual insertion of "Er," "Y'know," "I mean," "like," "man," "well," and so on.

Now, I should like to propose a theory. That theory holds that the crucial element of information is not novelty but redundancy. Extending the notion of information a bit further, I should say that information as content is unimportant to the study of linguistics but that information as language is linguistics. Also, when I say that our assimilation of information is based largely on redundancy, I mean that in several ways:

First is the notion of redundancy of structure (syntax and grammar) and of lexicon, the factors basic to our assumption that we are, indeed, speaking the same language.

Second is the notion that language is redundant - reflective, if you like - of experience, and here experience should be construed in its broadest possible sense.

Third is that notion that within language, notwithstanding the huge number of possibilities for expression, speakers habituate to an infinitesimally small segment of the possible and, basically, spend most of their time speaking and writing about the same things. To be sure, there are many articles, books, speeches, conversations, and so on that have never been written or uttered before, but it must be recognized that only the rarest writings and utterances either contain an entirely new idea or are expressed in an entirely new way. If the latter were the case - that is, if the pattern were sufficiently unfamiliar - we should have great difficulty in understanding and assimilating the information. Witness certain kinds of poetry, certain kinds of writing - like Finnegans Wake. The tendency is to instruct people to "free their minds" when trying to assimilate such material, and "freeing one's mind" consists simply of throwing off, to the best of one's ability, the restrictive clichés that have become habitual for all of us. Clearly, the ability to

free the mind varies greatly and depends to some extent on what we have come to call by the names intelligence, sanity, imprinting, and so on. Among what we recognize as normal people, experience teaches, from the ability to learn that fire can provide a painful sensation to the ability to understand Gravity's Rainbow. I shall avoid complicating the issues presented here by bringing in music and fine art, but I do believe that much of what I have to say is applicable to them, too.

Applying this notion of redundancy to my third example I gave at the beginning of this paper, I offer the following information:

ned
EAT
INS

Meaningless? Now let me tell you that it is printed on a piece of pasteboard torn from the end panel of a box. All that can tell you is that the message - or information - is incomplete, a piece of news you might greet with some feeling of relief. There is little or not redundancy in this information, chiefly because the complete original contains very little of what I would consider to be redundant. Unlike my earlier example, BA & G LE, which was rife with redundancy, this pasteboard enigma yields no information that can be supplied by experience - at least, not by mine. I should go one step further and say that if we knew all of the words of English beginning or ending in ned, EAT, and INS, and, further, if we could guess that the missing parts of each word consist of three characters each, we might examine all possible permutations that could provide logical words when filled in. Even so, I would defy most people to select the proper missing characters. Before I provide the answer to this puzzlement, let me point out that even when complete the words are not readily recognizable as an established pattern in the same way that BY RU on a candy wrapper might provide an easy clue to its source: a BABY RUTH candy bar.

Since you are obviously unable to contain your curiosity any longer, I shall now reveal that the complete information on the pasteboard is:

Stoned
WHEAT
THINS

And, lest you think they are a reducing biscuit made to an Alice B. Toklas recipe, let me say that the "stoned" refers to the milling process used in grinding the wheat. "Thins" is self-evident. Let me go back a minute. May I see a show of hands of those who understood the Alice B. Toklas reference? Thank you. For the unitiated, Miss Toklas, who was, of course, Gertrude Stein's companion and friend, once baked some cookies for Picasso when he visited, and the cookies contained a heavy lacing of marijuana. I trust that the rest of the reference is clear.

And now perhaps you will understand what I mean when I say that there is no such thing as cognition, only recognition.

Familiarity, far from breeding contempt, breeds comfort and security. Admitting ignorance is tantamount among many - too many - to admitting stupidity; yet, when one analyzes content he must be thoroughly aware of experiential shortcomings. Memory plays a big part in recognition, just as it does in experience. Recognition is nothing more or less than the remembrance of things past - your things, my things, all things; linguistic and other events and other events both interpretable and uninterpretable in linguistic terms.

As you all well know - in other words, what I am about to say is redundant

- we (we lexicographers, that is) have been known to talk about the meanings of words being the sum of their contexts. Context must be interpreted very broadly, indeed, for such an observation to have any validity. But for the purpose of this paper, I shall confine myself to linguistic context in utterances - written and oral. I should, however, like to make even more narrow the concept of context, for I believe that it ought to be extended to include such partial words and expressions - clichés, if you like - as I have been discussing.

You are in a theater. The houselights are dimmed and the curtain rises. At various positions along the walls of the auditorium red lights become prominent in the darkness. As you eyes begin to adjust to the gloom, you note that four red signs along the left wall read EXIT; along the right wall there are three that read EXIT and one that reads E IT. Take a wild guess at what the obliterated or unlit letter is. D? No, guess again. M? No, still wrong. Certainly, no one has to examine the sign from close up to be quite certain that it an X that remained unlit. Another BAR & GRILLE? Well, yes, in a way, but this time the redundancy demands little recollection for context, even if you've never been in a theater before.

Analysis will show, I think, that the mind is able to go from the general to the specific and back to the general with ease. Returning to the burnt child that dreads the - guess what? - for a moment, we have a kind of circularity represented by an arrangement of this kind:

FIRE	warmth
	light
	boiling teapot
	roast meat
	burnt finger

Fire has many more associations than these, but it is only the last that gives rise to such wise admonitions as "Don't play with fire" and "Don't play with matches." "Once bitten, twice shy" is another maxim dealing with human experience. All are used metaphorically, which is just another way of describing generalization. An example of specification (from a generalization) is the ability to fill in the missing letters for BAR & GRILLE and EXIT.

One of the interesting aspects of this subject and one I have implied in my example of Stoned WHEAT THINS may be exemplified by the fact that no matter how sure you are that the word over the door in the theater reads EXIT, you cannot be absolutely certain until either the bulb behind the X is replaced or you examine the unlighted cutout of the sign. Hence, what we are dealing with is probability and not incontrovertible fact. I find probability a fascinating adjunct to the study of language. Turning to lexicon for a moment, let us say that we have an imaginary dictionary in which the ordering of definitions is in absolute frequency order, an order based on vast statistical evidence. In this dictionary appears the word court followed by these definitions (which I have done informally here):

1. an enclosed, roofless area..... 40%
2. a short street..... 30%
3. a level area marked off for a game 20%
4. a place where justice is dispensed 10%

(Since this is an imaginary dictionary, I won't entertain any quibbles regarding the percentages.)

Now we come to dictionary user number 1, who has just encountered the utterance, 'Jones was taken from the court on a stretcher.' Looking up the word court, he reads the percentages; no matter which one he selects besides the first definition he may be wrong. I, being omniscient, know that the right answer is 3, because

I know that the utterance was about "Gutsy" Jones, the great tennis player. User number 2 encounters 'The basketball court was completed in time for the championship game.' The question to which I should like to address myself is, How does the dictionary-user know which sense is appropriate? The ready answer is that the wrong definitions yield nonsense, but that is begging the question. In a mechanistic approach, we might say that the user sorts all of the four possible definitions, one by one, against the context and matches up the correct one. That may well be, but how does he determine and what are the criteria for correctness?

If one assumes a tabula rasa, the problem becomes unanswerable and bootless. In any case, people don't have tabulae rasae - only computers do - and there is little to be gained in establishing an unnatural situation in order to answer a natural question.

I suggest that what happens is something like the following:
(1) Frames are established in the user's mind. These frames are quite large in number but, for a given language, are finite. (As an aside, I might that the ability to assimilate an inestimable number of foreign languages would seem to indicate that human capacity for learning structural frames is very great, indeed.)
(2) The frames are learned from infancy onward, and there would appear to be at work an associative capacity of the human brain enabling it to classify such frames into however many categories of them may exist. I venture to say that it may be possible that such conditions as aphasia and dyslexia are reflexes of an impairment of this associative function, whether that impairment be physical, chemical, or neural.

At first examination, it seems surprising that people are able to learn to speak a language, but if you consider the millions of times that language frames are encountered in the everyday business of living and communicating, it would be equally surprising if they didn't. On the other hand, there is everything to suggest that animals possess this higher-level associative capacity in only a very limited way.

To support this idea of the dependency of language acquisition on this associative network, I should point to the learning curves of children acquiring their native language with those of children learning a foreign language: the early part of the curve is gradual; then, after some apparent "breakthrough" point, the curve rises more steeply until it begins to level off. I submit that the first, gradual increase in proficiency is related to the apprehension of basic modular frames, that the steeper rise is attributable to the period during which the associative network goes - so to speak - into full operation, and that the final leveling-off can be described as that period during which the majority of the commonest frames will have already been assimilated and further acquisition is limited chiefly to style, lexicon, and the occasional frame that was not in the pattern earlier because it might be encountered only rarely.

The same system operates in the acquisition of a foreign language, though, clearly, that acquisition is greatly accelerated if the number of frames encountered is increased. This increase may come about in several ways: first, if the languages are related, learned frames in one may apply to the other; second, the exposure to frames can be increased artificially - something usually called "studying" - or more naturally, by placing the learner in a situation where he is exposed to little or nothing but the foreign-language frames.

If you accept this hypothesis, then consider the frequency, the redundancy, the repetition of language with which we are faced daily. Consider, too, that the acquisition of language by, say, Helen Keller, was attributable to her innate human ability to assimilate frames. It made no difference which frames were assimilated, you understand, for the brain cannot distinguish between Chinese and Quechua. The

only important thing is that there be a frame of some kind - a symbological structure.

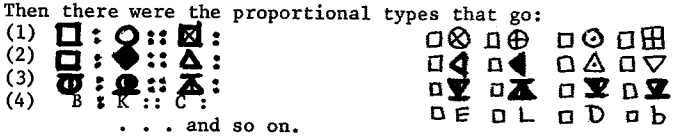
The most common symbological structure we know of is language; yet we know of abstractions far less familiar that test the hypothesis. Remember those multiple-choice tests? They contain questions like the following:

Complete the following series:

(1) 1, 2, 3, 4, 5	□7	□14	□6	□71
(2) 2, 4, 6, 8, 10	□12	□14	□6	□72
(3) 1, 5, 4, 8, 7	□6	□11	□4	□3
(4) 4, 7, 13, 25, 49	□97	□68	□89	□91

. . . and so on.

Then there were the proportional types that go:



. . . and so on.

The ability to solve these - and other problems of much greater subtlety - is attributable to the associative powers of the human brain, the capacity to fill in gaps, the natural proclivity for finding system and form, sometimes even where only chaos exists. Although it is true that these capabilities vary among us, the vast majority have the ability to assimilate language. Those who fall short of that ability - aphasics, dyslexics, and others - are recognized as abnormal; some of those who possess the ability to an extraordinary degree are among our geniuses; still others, though, while they may possess an extraordinary ability for associative understanding, might be quite inadequate in their other mental capabilities, and here I would classify the idiot-savants and other rare occurrences of similarly impaired function.

Returning to my primary thesis of information frames, it may now be seen how the associative capacity, fed and reinforced by redundancy, offers us the ability to provide missing information. It would be foolish to maintain that all information can be patterned in the same way for all human beings, however. For example, it can be demonstrated that if all the members of an audience were to be asked the color of a speaker's pocket handkerchief (which he surreptitiously tucked down out of sight a moment or two earlier), many would be unable to do so. Observation of ostensive objects, I believe, may be classified into the same category, for we know that people can be trained to observe. Moreover, the memory courses offered to the public are generally based on the assimilation of a set of easily learned frames onto which unfamiliar or disparate facts and figures can be hung.

Were I to tell you that I remember the year of American independence as 1776 very easily only because it came 140 years after the Flushing Remonstrance, or 1112 years after the Synod of Whitby, or 2131 years after the death of Xenophon, you would assume either that I was joking or that my frames of reference were very peculiar.

On the other hand, I can provide an example of another kind. I'll wager that I could ask each of you a simple question about something with which you are intimately familiar, to which you know the answer, but to which you would be unable to give me a truly quick reply. (By a "quick reply" I mean the kind of answer you could provide to a question like "What is your name?") The question is simply this: "How many letters are there in your entire name?" Try it on your friends - most would have to count on their fingers!

I have tried in a number of different ways to examine and to help explain some of the everyday phenomena we all seem to take for granted. You may argue that I am really less concerned with information than with paucity of information, and you may be right. How we know things, how much information that we consider to be new is really "old" - within us, so to speak - and how much of what we, as normal adults, supply when we are at the receiving end of communication - all these matters have seized my interest and attention. And I hope they have yours, as well.

Essex, Ct. 06426

ON THE EARLY ACQUISITION OF KINESICS*

Walburga von Raffler Engel
Vanderbilt University

No descriptive account of first language acquisition can be adequate without a parallel description of kinesics.[1] Theories which treat the development of language as if it were an entity totally separate from all the other means of communication used and understood by the child are incomplete at best, and more frequently, of little or no value because they are misleading. The infant becomes aware of other human beings as soon as he is born; his social interaction with others starts soon thereafter. The child's communicative behavior is indeed the criterion on which we rank whether he is normal or not. Deafness and blindness are grave handicaps but autism is infinitely more severe.

That most children say bye-bye before they say hello (and this is true cross-culturally[2]) is explainable by the fact that whereas a smile frequently accompanies the greeting formula 'hello', there is generally an added waving of the hand when one sayd 'goodbye' to a child. The more consistent and more marked gesture of waving reinforces the verbal formula. Accordingly, many children wave their hand before they can say bye-bye. It is possible that they select the easier of the two modalities. It follows that the child does not perceive the meaning as contained solely in the words, because if this were the case he would not use the gesture except as an accompaniment to the words. It appears more likely that unconsciously the child considers the words and the accompanying gesture redundant.

Verbalization then, even though the most important one, is only one facet of human communication. Vocal non-verbal paralinguistics, posture, gesture, facial mimicry and eye movement, proxemics, chronemics, and the whole gamut of kinesics and body movement constitute a vital part of the expressive aspect of the speech act. They also are part of the clues for understanding it. This observation runs contrary to the radical nativism proposed by Chomsky which is based on a shallow logical scheme strictly derived from certain aspects of a certain brand of syntax exclusive even of intonation contours. To the unbiased observer, however, this observation is rather self-evident. What one has to discuss is the relative importance of each modality, both as a single entity and as to the cumulative effect of its components.

Essentially, non-vocal non-verbal behavior can be divided into five main categories. (1) Body movement communicates something about the mood of the speech partners and their general attitude towards each other but is not necessarily to be considered an integral part of the speech act proper. This is generally called body language. (2) Instinctive gestures, such as an expression of disgust on one's face when one sees the familiar old lady approaching who talks endlessly about her ailments, may be strictly involuntary and even cause one to be embarrassed. One can, nevertheless, exhibit such an expression purposely in order to get rid of an inopportune person. In the first instance

* The present paper builds on material presented at the Twenty-fifth Annual Kentucky Foreign Language Conference by the title "The Acquisition of Kinesics", April 1974.

the facial expression is not part of the speech act while in the second it is. (3) Semiotic gestures are culture bound and learned and would therefore appear to be the opposite of instinctive gestures. There is, however, a strong cultural bias in the expanse and in the extent to which people will freely give in to instinctive gestures. Many semiotic gestures are to a large extent based on features having universally understandable components. This is particularly true of obscene gestures. Although the latter vary among cultures it is fairly easy for the outsider to grasp their meaning. (4) Paralinguistic gestures accompany verbalization and are almost always part of the speech act. A typical paralinguistic gesture is lifting one's hands and putting the two forefingers at a close distance while saying that such and such an object was very small. Paralinguistic gestures are frequently essential to the message. As Bolinger states, "facial gestures are sometimes the only way to tell a question from a statement."[3] (5) The same holds for descriptive gestures, such as pointing at an object, or twirling one's hand when mentioning a circular stairway.

The boundaries of the five gestural categories are not clear cut. Verbal and non-verbal aspects of communication are sometimes redundant and compensatory at other times. In analyzing the total impact of a message on the hearer it is far from easy to separate verbal and non-verbal factors.

This extremely complex picture would favor a theory of first language acquisition that starts from the infant's primary means of communication rather than one that considers a sharp break between the prelinguistic stage and the beginning of holophrasis.[4] It also points to the need for a more ethological approach to language acquisition and a greater emphasis on mother-child interaction. Lately, there has been some resurgence of interactional studies among developmental linguists as well as psychologists due to the decline in prestige of the generative-transformational school of psycholinguistics.

The process of socialization begins at the very moment of birth and thus the study of language acquisition belongs obviously as much in the realm of sociolinguistics as it belongs to psycholinguistics. The ethological approach to developmental linguistics takes into account the importance of a loving interaction of the infant with an adult. This approach also does not isolate verbalization from the non-verbal means of communication. The co-occurrence of the two modalities is particularly apparent during the first two years of life while brain lateralization is gradually taking place. To express their need for food, babies cry louder and kick harder. Babies react to soothing words as they react to a comforting touch. Most interestingly, babies distinguish communicative speech from non-speech vocalization. This has been verified empirically by myself,[5] and with great accuracy and detail by Condon and Sander.[6]

The Condon and Sander study yields extremely important results. They document that "the quantal forms of microorganization of a speaker's motion are isomorphic with the articulate structure of his speech." The Condon and Sander term for this kinetic behavior is <u>self-synchrony</u>. The hearer also adjusts his body motion to the rhythm of the speaker in <u>interactional synchrony</u>. Condon and Sander have observed interactional synchrony in five normal neonates revealing "a synchronization of infant movement organization with the articulatory segments of adult speech as early as the first day of life." Depending on degree of severity, I have observed that brain damaged infants do not distinguish

speech sounds from non-speech vocalizations. The research by Condon and Sander shows that mentally defective babies reveal interactional asynchronies.

The point of this paper that language acquisition cannot be adequately investigated when it is arbitrarily isolated from the other modalities of communicative behavior is also supported by the extensive research on mother-child interaction conducted by Margaret Bullowa, particularly in her latest work on the synchronization of the communicative behavior between infants and adults.[7] According to Bullowa "this aspect of development is a necessary precursor to language acquisition because the patterning of speech (a neuromuscular activity) has to fit into the ongoing patterning of the rest of the body's motor activity in a very precise way. The infant must develop something like the adult pattern of hierarchically nested segmentation of his behavior at a number of levels."

Monolingual children grow up in a one-to-one rhythmic relationship between language and body movement. Bilingual children probably have two such one-to-one relationships, but very little is known about the kinesics of bilingual children and any statement about them would be premature. It is therefore better to restrict the content of this paper to monolinguals. They "tune in" correctly and are "on the same wave length" with their socio-cultural peer-group. Social and dialectal differences may cause feelings of hostility because their speech pattern "rubs them the wrong way."

We know that speech tempo varies among languages. Speakers of Romance languages produce more syllables per second than speakers of the Germanic tongues. Yet of the body rhythm we know next to nothing. It is possible that this is what causes more cross-cultural misunderstandings than other factors of which we are more aware, and to which we may be less sensitive. The violation of our customary private space or of our public space, the straining of our ears by listening to a language that is spoken more softly or louder than ours may be far less irritating than when we feel forced into an unaccustomed body rhythm by the demands of interactional synchrony.

Foreign language teachers have always complained that linguistics does not give them enough useful information. Maybe they just don't know how many more problems they would have if they really studied linguistics . . . They may have to enlist the gym coach and members of the drama department to help teach their beginning classes.

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COHESION AND REFERENCE IN SCHIZOPHRENIC SPEECH

James R. Martin and Sherry Rochester
Clarke Institute of Psychiatry, Toronto

This paper is divided into two main parts which are concerned with the application of two types of linguistic description to schizophrenic speech. The sample of discourse is taken from interviews conducted in the past year with 40 schizophrenic subjects, 20 thought-disordered and 20 non-thought-disordered, as well as 20 normal speakers. The corpus represents three different contexts of situation for each subject. In one he is repeating in his own words the fable of "A Donkey Carrying A Load of Salt" which has just been read to him. In the second he is describing what he sees in a set of cartoon pictures and explaining why they are funny. In the third, he is conversing with an interviewer, and the text is generally selected so that he is not interrupted by the interviewer in this situation. These contexts will be referred to as "the donkey story", "the cartoon task", and "the interview" respectively.

The first description considered will be that of cohesion in the subjects' discourse. The second will consider one aspect of cohesion, reference, in more detail and from a complementary perspective. The results will be of three kinds, revealing: 1. differences between groups; 2. generic differences between contexts; and 3. the sort of contribution such descriptions make towards an understanding of how the textual function of language creates or sometimes fails to maintain "texture" in English discourse.

Part I

One approach to the problem of describing texts which are more than a single sentence in length is to demonstrate why some piece of discourse is easily distinguished from a string of unrelated sentences which do not form a single text. In Halliday (1964), Hasan (1968), Gutwinski (forthcoming), and Halliday and Hasan (forthcoming) is developed a comprehensive framework for investigating non-structural relations between sentences, or "cohesion" in a given text. Halliday and Hasan describe cohesion as responsible for establishing continuity in a text and argue that "the ongoing continuity of discourse is a primary factor in its intelligibility" (chap. 7, p. 16).

In the present study, cohesion is analyzed in language which is at times difficult to follow or understand. In particular, the analysis is concerned with formalising in linguistic terms a description of so-called "process thought-disorder" in schizophrenia. The speaker with this disorder is described (Bleuler, 1950) as talking in a loose way with "wooly" vagueness, and with an inconsequential following of side issues away from the main topic of conversation. Another part of the present study has provided clear evidence that native speakers judge many segments of thought-disordered speech disruptive to the coherence of a text; in contrast, judges found few disruptive segments in the speech of non-thought-disordered schizophrenic patients and normals. That is, judges find the speech of thought-disordered schizophrenic speakers disrupted while they have little trouble in following the speech of other (non-thought-disordered) schizophrenic speakers or normals.

In view of these differences in the experience of native speakers it was hypothesized that an analysis of cohesion would reveal differences

between thought-disordered speakers and other speakers, where the other speakers were both schizophrenic and normal. In addition, it was expected that some levels of analysis would simply reveal differences between schizophrenic and non-schizophrenic speakers. These differences while noteworthy, may not represent differences between normal and schizophrenic speech alone. Schizophrenic subjects are typically disturbed and upset when interviewed while normal subjects are generally at ease.

Halliday and Hasan discuss cohesion under five headings and their description has been adopted in this study. The five headings are: Reference, Substitution, Ellipsis, Conjunction, and Lexical cohesion. These categories will be briefly described and additional examples appear below.

Reference is a semantic relation whereby information needed for the interpretation of some item is found elsewhere in the text. Reference may of course be exophoric, to some object in the situational context of an utterance, but only intra-textual, or endophoric reference is cohesive. Pronominals, demonstratives and the definite article, and comparatives are the main exponents of this type of cohesion. In saying "John went downtown and later he returned home", "he" refers back to "John" in the previous clause. This relation between "he" and its presupposed item constitutes one cohesive tie. In this analysis, cohesion was analysed between independent clauses, whether adjacent or not. Cohesive relations within such clauses were not considered, since within a clause, structural as opposed to non-structural unity is dominant.

Substitution consists of replacing a nominal, verbal, or clausal element with substitutes like "one", "do", or "so". In saying "John sold his old racquet and I sold one too", "one" presupposes "old racquet" and constitutes a cohesive tie. Ellipsis is really substitution by zero. In "I was born in New Brunswick but live in Ontario", the gap in the second clause presupposes "I" in the first.

While substitution and ellipsis are lexico-grammatical relations, conjunction expresses a logical relation between clauses. The presuppositional constraints governing the use of conjunctions are not as yet well understood but conjunctions were analysed as cohesive ties in this research. Lexical cohesion involves the repetition of an item formed on the same root or a synonym or near synonym. Of course, there is a real gradience here from similar to opposite meaning. A fairly conservative approach, ignoring for example patterns of collocation in a text was adopted in this study.

Reference	1. pronominal:	We met Joy Adamson and had dinner with *her* in Nairobi.
	2. demonstrative:	We went to a hostel and oh *that* was a dreadful place.
	3. comparative:	Six guys approach me. The *last* guy pulled a knife on me in the park.
Substitution	1. nominal:	The oldest girl is 25 and the next *one's* 22.
	2. verbal:	Eastern people take it seriously. At least some of them *do*.
	3. clausal:	I'm making it worse for myself. I would think *so*.

Ellipsis	1. nominal:	He's got energy too. He's got a lot more ϕ than I do.
	2. verbal:	I could go to university all my life, ϕ keep going to school.
	3. clausal:	My father took me from Ireland and ϕ put me in the Children's Aid.
Conjunction	1. additive:	I read a book in the past few days *and* I like it.
	2. adversative:	They started out to England *but* got captured on the way.
	3. causal:	It was a beautiful tree *so* I left it alone.
	4. temporal:	My mother was in Ireland. *Then* she came over here.
	5. continuative:	What kind of degree? *Well,* in one of the professions.
Lexical	1. same root:	Mother needed independence. She was always *dependent* on my father.
	2. synonym:	I got angry at M. but I don't often *get mad*.
	3. superordinate:	I love catching fish. I caught a *bass* last time.
	4. general item:	The plane hit some air pockets and the bloody *thing* went up and down.

With reference to Table 1, below, consider first the application of the analysis to the donkey stories for each group. The figures in the table represent the mean percentage of ties in each of the five cohesive categories for each group in two contexts. In narrative, the similarities between groups are striking: reference accounts for almost half the cohesive ties, lexical cohesion for 30% and conjunction for 20%. Substitution and ellipsis are insignificant in this genre. Schizophrenic speakers thus appear able

TABLE 1
DISTRIBUTION OF COHESIVE TIES IN TWO CONTEXTS OF SITUATION

Group	Context	Proportion of Ties Per Category					Total Cohesive ties for average S	# Ties per Independent Clause
		Ref.	Sub.	Ell.	Conj.	Lex.		
Thought-disordered Ss N=20	Story	.48	.00	.01	.21	.28	29	2.94
	Interview	.31	.01	.04	.22	.41	42	1.93
Non-Thought-Disordered Ss N=20	Story	.48	.01	.03	.19	.27	29	3.14
	Interview	.31	.01	.08	.28	.32	42	1.92
Normal Ss N=20	Story	.46	.00	.02	.20	.30	44	3.79
	Interview	.32	.00	.04	.31	.33	52	2.24

to do the same things as normals do. However, the last column in the table reveals that they cannot do so as often. The stories were edited for each group so that only material relevant to the narrative was included. So changes of topic or losing the train of thought are not affecting the scores, and therefore not distinguishing thought-disordered from non-thought-disordered schizophrenic speakers. In terms of the number of cohesive ties per clause, there is thus a significant difference only between normal and schizophrenic speakers, the latter using about 25% fewer ties per independent clause.

In the interview situation, groups differ in the kind as well as the amount of cohesion used. Here, all non-thought-disordered subjects, both normal and schizophrenic depend more on conjunction and less on lexical cohesion than thought-disordered subjects. Again, normals use significantly more cohesive ties per clause than schizophrenic speakers. These results demonstrate that an analysis of cohesion at least partially reflects the fact that native speakers find many more disruptive segments in the speech of thought-disordered patients. As well, it may be that conjunction, expressing as it does logical relations between sentences, creates stronger cohesive bonds than does lexical cohesion which thought-disordered subjects depend upon more heavily than the other groups. In most discussions of cohesion, it is assumed that each instance of a cohesive tie makes about the same contribution to creating texture in discourse because there was no way of measuring the strength of the various cohesive categories. If an analysis of cohesion is in fact reflecting the judgments of native speakers about the disruptiveness of certain discourse, then some evidence is provided for suspecting lexical cohesion to be weaker in cohesive force than conjunction.

Turning to generic differences revealed in the analysis, note that for all groups, reference is much more important in the donkey stories than in the interviews, constituting 14-17% more ties per clause in the narrative context. For normal and non-thought-disordered subjects, conjunction is 9-11% more important in the interview than the story. This data seems to indicate that the realisation of participants throughout a narrative is most responsible for continuity in such a text, while in conversation, the logical relation of the ideas realised in independent clauses becomes as significant as continued reference to participants or the repetition of lexical items. Note as well, that for all groups, interviews have 1 & 1½ fewer cohesive ties per clause than narratives. The loss of referential cohesive ties is not replaced through other cohesive categories. Conjunction and lexical cohesion become relatively more important, but overall, the amount of cohesion declines.

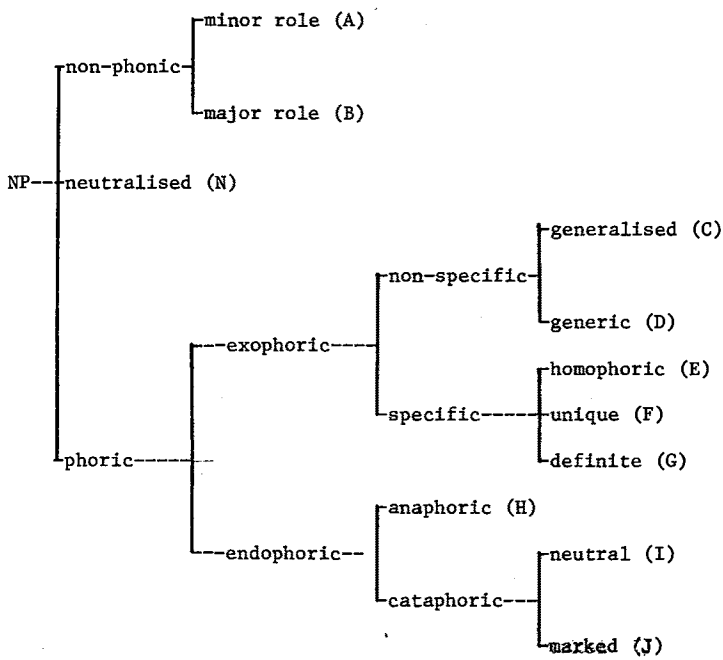
Part II

The analysis discussed in Part 2 approaches each context from the points of view of speaker and listener. It attempts to describe the way in which a speaker uses certain noun phrases to help signal presupposition and uses other noun phrases to introduce information to a text for the first time. The system of reference accounting for these facts is termed "phoricity". Because of space limitations, only a part of the research can be presented in detail here. Developing a framework for the analysis consisted of three main steps which are described below.

First, noun phrases were distinguished on formal and contextual grounds according to the type of reference they entail. The results are found in Figure 1. Only definite, anaphoric, and neutral cataphoric noun phrases will be discussed at this time. Certain major distinctions lead to these

FIGURE 1

PHORICITY: Types of Noun Phrase Reference in English



Minor Role	A. The first day he didn't have *a load*.
Major Role	B. He has *a glove* and the glove is extremely big.
Generalised	C. It's funny because *you* wouldn't expect to see a commuter on a ski lift.
Generic	D. It's funny because *mountains* can't wave.
Homophoric	E. There's a man on *the subway* reading a paper.
Unique	F. Here we have *Snoopy* with a butterfly over his head.
Definite	G. *This* is a picture of Mt. Rushmore.
Anaphoric	H. Here we have Charlie Brown and *he*'s waiting for a ball.
Neutral Cataphoric	I. Because *this* is how I feel that I need to go round the inferno once more.
Marked Cataphoric	J. There's *this donkey* and he's going across the river.
Neutralised	N. *Ball* thrown at *subject*. *Subject* received *ball*.

particular classes. The first is drawn between noun phrases requiring referents (phoric NP's), and noun phrases not requiring referents (non-phoric NP's). Phoric noun phrases may be then either exophoric when their referent is found in the situational context of an utterance, or endophoric when the reference is intratextual. Definite noun phrases make a specific exophoric reference to some object in the situation. In saying, "I am in the middle of reading this paper", "this paper" is such a noun phrase. Endophoric noun phrases are anaphoric when the referent precedes the noun phrase requiring it and cataphoric when the referent follows. "He" is an anaphoric noun phrase in "I see Charlie Brown and *he*'s standing there." "This" is a cataphoric noun phrase in "Because *this* is how I feel: that I need to go round the inferno once more."

Having established these types of reference made by noun phrases in English, the next step was to consider the function of such reference in discourse. The four criteria proposed for determining this function appear in Table 2. Basically, the potential of certain noun phrases to provide referents in a text is being contrasted with the fact that other noun phrases require referents. A positive response to each question is marked by a check on the table. Definite, anaphoric, and neutral cataphoric noun phrases all can and in fact must have a referent in the verbal or situational context of an utterance and can as well, although they need not, provide referents for other endophoric noun phrases. They are assigned a phoricity value of 4 representing this function.

After considering the different types of reference and their function in discourse, the last step involved examining the ways in which a listener retrieves referents for noun phrases with a phoricity of 4 which require them. In this part of the analysis only noun phrases which were not included in other noun phrases were considered. This was because many noun phrases possess referents in the noun phrase including both of them. For example, "the boy" in "the boy from Ottawa" is cataphoric, pointing ahead to "from Ottawa". As a whole, the noun phrase "the boy from Ottawa" does not require a referent, and does not function with a phoricity of 4. In this study only noun phrases needing a referent from outside themselves were examined. Thus "complex" in Table 3 must be understood as "not included in another noun phrase". Such noun phrases might or might not contain other noun phrases.

Two of a listeners' strategies for retrieving referents are implicit in much of the discussion above. With exophoric retrieval a unique referent is found in the situation. With endophoric retrieval it is found in the verbal context. Clark (in press) has characterised other strategies which are necessary when a unique explicit referent is not available. One he terms "bridging". For example, in saying "There's a house over there and a couple are standing in the door", no explicit referent is provided in the verbal context for "the door". Yet the listener recognizes which door is meant because "a house" provides an implicit antecedent. To say "There's a house and a couple standing in the door of the house" even sounds a little redundant. Most noun phrases seem to provide implicit antecedents for noun phrases that somehow fall within their semantic range. Clark has demonstrated that it takes listeners longer to understand sentences involving bridging than when endophoric retrieval is entailed. But speakers often introduce noun phrases in a phoric way even though only an implicit referent is available.

TABLE 2
FOUR CRITERIA FOR DETERMINING
THE REFERENTIAL FUNCTION OF NOUN PHRASES IN A TEXT

	Types of Reference									
	A	B	C	D	E	F	G	H	I	J
1. Can a referent for the noun phrase have been previously introduced in the verbal or situational context?	-	-	✓	✓	✓	✓	✓	✓	✓	-
2. Must the noun phrase have a unique referent in the verbal or situational context?	-	-	-	-	-	-	✓	✓	✓	-
3. Can the noun phrase provide a referent for an endophoric noun phrase?	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
4. Must the noun phrase provide a referent for an endophoric noun phrase?	-	-	-	-	-	-	-	-	-	✓
Phoricity Values (P)	1	1	2	2	2	2	4	4	4	3

Retrieval: Examples of Retrieval Categories

endophoric	Ren:	A donkey was loaded with salt and *he* went to cross a river.
exophoric	Rex:	*The car* is passing *the mountain*.
bridging	Rbr:	There's a house in the picture with two people standing in *the door*.
addition	Rad:	*The donkey* was crossing *the river*.
ambiguous	R?:	A commuter and a skier are on a ski lift and *he* looks completely unconcerned.

TABLE 3
PROPORTION OF NOUN PHRASES
ACCORDING TO REFERENCE FUNCTION AND RETRIEVAL STRATEGIES

Group	Context	Phoricity Values (P) 0	1	2	3	4	NP's (complex) For mean subject	Retrieval Categories Ren	Rex	Rbr	Rad	R?	NP's (complex) P=4 For Mean S
Thought-disordered Ss N=20	Story	-	.11	.11	-	.78	24	.68	.03	.11	.17	.01	19
	Cartoon	.02	.16	.18	-	.64	146	.63	.31	.04	-	.01	93
	Interview	.01	.12	.31	-	.56	55	.42	.45	.04	.07	.02	31
Non-thought-disordered Ss N=20	Story	.01	.12	.06	-	.81	22	.80	.01	.07	.11	.01	18
	Cartoon	.01	.18	.15	-	.66	142	.70	.21	.06	-	.01	95
	Interview	-	.13	.24	.01	.62	54	.39	.54	.02	.04	-	33
Normal Ss N=20	Story	-	.08	.10	.01	.81	28	.88	-	.11	.02	-	22
	Cartoon	-	.16	.22	-	.65	134	.80	.12	.07	-	-	87
	Interview	-	.16	.26	.01	.58	64	.50	.39	.08	.02	-	36

Another of Clark's strategies is termed "addition" and is operative when no antecedent at all is available to the listener. In such cases he must supply his own as best he can. A final retrieval category accounts for the fact that at times more than one referent is available and the listener cannot be sure which is intended. This is termed ambiguous reference.

With reference to Table 3, consider some of the results of this second approach to reference. The first general feature to be noted is that for each group in all genres, noun phrases with a phoricity of 4 are more common than all other types of noun phrases combined. Then note that for each group, such noun phrases are more common in narrative than in the cartoon task and more prevalent in the cartoon task than the interview. Turning to the retrieval categories some other interesting generic differences emerge. For all groups, endophoric reference is more important in the narratives than in the cartoon task, and more important in the cartoon task than in the interview. Conversely, exophoric retrieval is negligible in a narrative context, but more important in describing a picture. The even higher proportion of exophoric reference in the interview is mainly due to the extensive use by all speakers of first person pronouns which were analysed as exophoric. Note as well that in describing a picture, virtually no addition on the part of a listener is required, while the interview requires such retrieval and the story an even larger amount.

Consider now the differences between groups. In both the donkey stories and the cartoon task, normals demand more endophoric retrieval than non-thought-disordered subjects, and they in turn more than thought-disordered speakers. In the interview situation, normals require more endophoric retrieval than both groups of schizophrenic speakers. With respect to exophoric retrieval, the biggest differences are found in the cartoon task where thought-disordered subjects use significantly more exophoric reference than the other groups. Normals appear to use less exophoric reference than non-thought-disordered speakers in this task. The category Addition displays some other important differences. In both the story and the interview, thought-disordered speakers force their listeners to provide their own referents more than do non-thought-disordered speakers who in turn require more addition than normals.

What these results reveal is that thought-disordered subjects, and to a lesser degree, non-thought-disordered schizophrenics, are less likely than normals to provide antecedents in the verbal context of their utterances for noun phrases which require them. In describing a picture they depend more heavily on the situation to supply the referents they presuppose, and when speaking in narrative or in conversation, many more of their noun phrases simply have no referents at all. In other words, schizophrenic speakers are not providing the information necessary for interpreting one part of the presuppositional content of their utterances in the way normal speakers do. This conclusion seems to indicate that a more comprehensive examination of presupposition in schizophrenic speech is in order. Such a study would consider along with phoricity, two other systems which are integral to signalling presupposition in English: 1. the system of intonation marking information structure in the English clause as described by Halliday (1967-68); and 2. the structures whereby verbs presuppose their that-S complements or noun phrases their relative clauses as discussed in Kiparsky and Kiparsky (1968), Karttunen (1971) and Keenan (1972).

In closing it is important to put the findings presented here in perspective by discussing some of our failures. When the judges discussed above were picking out disruptive segments in the interview situation, they were presented with four passages of discourse. They felt that some of these contained many more disruptive segments than others. At one stage in our analysis we tried to see if the most disruptive segment for a given speaker was less cohesive or entailed more addition, more ambiguous reference, and less endophoric retrieval than the least disruptive passage. No evidence was found to confirm any of our hypotheses. It seems that while an analysis of cohesion and phoricity tells us some of what we expected to find, it is not reflecting absolutely what the native speaker feels about what constitutes smooth discourse in English. Putting our analyses in perspective, this is not surprising. Phoricity examines only part of the question of signalling presupposition in English. And cohesion is only one component in the textual function of language. Without an accompanying analysis of factivity, the marking of the information units "given" and "new" through intonation, or the significance and distribution of theme and rheme, we are viewing only a part of the story. Moreover, a native speaker has a feel for when a lot of cohesion is needed and when a little is needed, or when certain types of reference are appropriate and when they aren't. The only way I can imagine of getting at this question of density is to relate our grammatical studies to discourse structure on the semological stratum. But as yet, it seems barely possible to adapt a reticulum description of English narrative, let alone conversation. Finally, we have to formalise a description of how to change topic in discourse, since certainly we shift our field of discourse frequently without the disruptive consequences we find in schizophrenic speech.

In spite of these difficulties, I hope this paper has provided us with some greater understanding of how text is created in certain genres and how this process is breaking down in schizophrenic speech.

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UNDERSTANDING AMBIGUOUS SENTENCES[1]

Douglas Vipond[2]

York University, Toronto, Ontario, Canada

ABSTRACT. This paper reviews psycholinguistic data and theory to answer the question: How do people comprehend ambiguous language? According to the Trial-and-Error Hypothesis, we process only the most probable meaning of ambiguous input, whereas the Exhaustive Computation Hypothesis asserts that we process, at some level, all major meanings. Both hypotheses have been supported in the literature, but studies in favor of the Exhaustive Computation view are somewhat sounder in methodology. Subjects apparently use the Exhaustive Computation strategy even when context indicates which meaning is correct; however, a strong "set" for a certain structure may over-ride the ambiguity effect. The research with ambiguous sentences is then set within a larger framework by comparing two models of language comprehension. Dictionary Models take a "subsystem" approach; they emphasize the role of the subjective lexicon and distinguish between dictionary and encyclopedia knowledge. Network Models represent all information in the same (associative) form, and do not draw a line between word and world knowledge. Although Dictionary Models may be appropriate for semantic theory, for the purposes of a psycholinguistic model of comprehension it is argued that Network Models are more useful. This is because: they represent the continuity of linguistic and nonlinguistic knowledge; and they account for the importance of psychological elements in language performance. Similarities between Network Models of memory and network approaches to language are noted.

One of the peculiarities of language is that a single linguistic unit may be interpreted in two or more distinct ways. This phenomenon, AMBIGUITY, has proved useful both in linguistics and psychology. Linguists have found ambiguity expedient in a variety of ways. Katz and Fodor (1963) required that a semantic theory be able to recognize and resolve those ambiguities a fluent speaker could. Chomsky (1965, 1972) used ambiguous sentences such as 1 to illustrate his distinction between deep and surface structure:

1. I disapprove of John's drinking.

Bennett (1968) argued for a stratificational view of language partly on the evidence of ambiguous prepositions: e.g.,

2. He wrote a book <u>on</u> the Queen Mary.

For a thorough linguistic description of ambiguity, see Kooij (1971). Meanwhile, psychology has been exploiting ambiguity ever since the Gestalt psychologists made ambiguous drawings (e.g., goblet/profile; old woman/young woman), demonstrating that perception involves the internal organization of the perceiver as well as the properties of the stimulus.

Within the last decade or so, psycholinguists have begun to explore the "information processing" aspects of ambiguous language. There are several reasons for this interest. First, the ubiquity of ambiguity automatically requires any theory of language use to deal with it. Second, one can argue (as Garrett 1970 did) that most utterances in natural language are open to more than one distinct interpretation, as least given an extended context. Or, one can say most nonambiguous utterances are "locally ambiguous" since they usually contain at least one item that, by itself, can be interpreted in several ways. Thus there are good reasons for investigating the comprehension of ambiguity, and by doing so, psychologists hope to learn about language understanding in general.

There are several specific questions to be asked about ambiguity as a problem in language performance: Do people perceive one or more than one meaning in ambiguous sentences? Does ambiguity put additional processing demands on the listener? If so, does contextual information affect these demands? This paper (1) reviews the psycholinguistic data that bear on these questions, and (2) puts the findings within the framework of two more inclusive models of comprehension.

1. SURVEY OF RESEARCH

Before discussing specific experiments with ambiguous sentences, it is worthwhile considering what consequences ambiguity could have on comprehension. First, it might have no effect. According to this view, ambiguous sentences are treated exactly like nonambiguous ones. Just one meaning - the most probable one - is processed and perceived. Probability may be jointly determined by discourse and immediate context, the social-physical situation of the utterance, expectancy, frequency, and so on. If only one meaning is processed, the presence of ambiguity should not affect speed of comprehension. Processing speed is slowed, however, if the person inadvertently makes an interpretation that does not fit with later context. Realizing

his error, he then reprocesses the ambiguous unit, at some cost in computational speed. This model, which may be called the "Trial-and-Error Hypothesis," is attractive because it is in accord with several commonsense notions about ambiguity. It explains, first, why people are seldom aware of ambiguous language. At the same time, it accounts for a certain type of linguistic error and the "double take" effect it causes; e.g., in

3. The old man's glasses were filled with sherry.

(The sentence is from Schank 1972, whose computer program for comprehension works by trial-and-error.)

An alternative to Trial-and-Error is the "Exhaustive Computation Hypothesis," which asserts that we process, at some level, all major meanings of an ambiguous unit. Specifically, a sequence of operations is hypothesized: first, activation of all meanings; second, evaluation of all meanings; and finally, selection of a single interpretation (on the basis of context, expectancy, etc.). The Exhaustive Computation Hypothesis predicts ambiguity will have a detrimental effect on processing speed because you have to consider more meanings in an ambiguous than in a nonambiguous utterance. In short, there are two broad hypotheses of ambiguous sentence comprehension. One says we process just one meaning, with no increase in computational load unless reprocessing is necessary. The other says several meanings are processed and ambiguity does increase the processing time required.

Both hypotheses, however, appear to be supported by the experimental literature. The typical research strategy in this area is to have subjects read or listen to ambiguous or nonambiguous sentences, then perform some additional task. If performance on the task is worse following ambiguity, that is taken as evidence that the ambiguous sentences required more complex processing than their counterparts. For example, Foss, Bever, and Silver (1968) read ambiguous (e.g., 4) and nonambiguous (e.g., 5) sentences to subjects. The ambiguous sentences contained either lexical, surface structure, or deep structure ambiguities. At the end of each sentence a picture was projected. Subjects were asked to judge whether each picture represented the meaning of the sentence they had just heard.

Picture 1 (Boy and Road)	Picture 2 (Boy and Map)
4a. The boy is looking up the street.	4b. The boy is looking up the street.
5a. The boy is gazing up the street.	5b. The boy is looking up the address.

Table 1. Sample of stimulus materials (from Foss et al. 1968)

Foss et al. reasoned that if people process both interpretations of an ambiguity, both will be available at the time of the picture task; therefore verfication time should be as fast following ambiguous as nonambiguous sentences. Alternatively, if subjects initially compute just one meaning, verification will be fast only if the meaning represented by the slide is the one they perceived. If the meaning given by the picture is the one they did NOT see, reaction time should be slow. This is because subjects now realize this alternative meaning is also a valid reading of the sentence, and it is this extra "reprocessing" stage that delays verification time. Results supported the hypothesis that only one meaning is perceived initially. Subjects were slow to verify a picture only when they needed to reprocess a sentence. Foss et al. interpreted this experiment as evidence for the Trial-and-Error Hypothesis: given an ambiguous input, we initially process only the most probable meaning. Reprocessing takes time, but it occurs only if later information indicates an incorrect decision was made.

Cairns (1973) also found support for the Trial-and-Error view. She read pairs of sentences to subjects and asked them to judge whether each pair "went together." The first sentence (S1) of each experimental pair was in fact lexically ambiguous. The second (S2) resolved the ambiguity. In a pre-test, the "bias" of the ambiguous sentences was determined, where bias was operationally defined as the percentage of subjects seeing each meaning first. Some ambiguous sentences had one highly likely and one highly unlikely interpretation; in others, the probability of each meaning being seen first was about the same. The second member of each pair (S2) could resolve the ambiguity in either (a) the likely or (b) the unlikely direction. An example is:

6a. S1 The patient was irritated by her /weyt/ in the doctor's office.
S2 She had gained five lbs. since her last check-up.

6b. S1 The patient was irritated by her /weyt/ in the doctor's office.
S2 When he apologized for the delay she was no longer upset.

Like Foss, Cairns reasoned that if subjects perceive only the likely meaning of an ambiguous S1 then an S2 in the unlikely direction should force reprocessing and increase reaction time (Trial-and-Error Hypothesis). On the other hand, if two meanings resulted from the initial processing of S1 (Exhaustive Computation), no reprocessing should be necessary when S2 is heard. According to Cairns, then, the Trial-and-Error Hypothesis predicts a reprocessing effect for some ambiguous sentences, but the Exhaustive Computation Hypothesis predicts no such effect. Results supported the Trial-and-Error view.

A reprocessing effect was found when S2 resolved S1 in the unexpected direction. Not surprisingly, this effect was strongest when the likely meaning was HIGHLY likely (and therefore the unlikely meaning highly unlikely). The effect was weaker, but still statistically significant, when the two interpretations were about equally probable. Cairns concluded that people initially process only one meaning for both types of lexical ambiguities.

These experiments - by Foss et al. and Cairns - are, however, open to an important criticism. They assumed that if the Exhaustive Computation Hypothesis were correct, both meanings would be available at the time of a later task. This is a questionable assumption. All the Exhaustive Computation model says is that both interpretations are considered, at some "preattentive" level. But only one meaning is CONSCIOUSLY perceived;moreover, the semantic decision is made quickly. Therefore, even the Exhaustive Computation Hypothesis must predict a reprocessing effect if later evidence indicates the incorrect decision was made. Neither Foss' picture verification task nor Cairns' compatibility-judgment task, then, allows a clear choice between competing hypotheses. In both, there is too long a delay between the ambiguous sentence and the task itself.

How can the interval between sentence and task be reduced? One way is for subjects to read a sentence fragment and make up an appropriate completing phrase. This sentence-completion paradigm was first used in the study of ambiguity by MacKay (1966), and later by Mistler-Lachman (1972) and Bever, Garrett, and Hurtig (1973). Results from the three experiments are compatible with one another, so here I will consider only the study by Mistler-Lachman. In her Production Task, subjects were shown a stimulus sentence and were required to make up a sentence that followed reasonably from it. The stimulus sentences appeared in isolation or were preceded by a neutral context sentence. Stimulus sentences were nonambiguous or ambiguous at one of three levels (lexically, surface structure, deep structure). Thus in the Production-with-Context task, subjects would first read a context sentence (e.g., 7), followed by an (8) ambiguous or (9) nonambiguous stimulus:

7. (Context)	You have wrongly accused me.
8. (Ambiguous stimulus)	I was not feeding her dog biscuits.
9. (Nonambiguous stim.)	I was not giving her stale bread.

The interval from the presentation of the stimulus to the onset of the subjects' response was measured. Latency times were faster when a context was given, but more important, ambiguous sentences had longer latencies than their nonambiguous controls. In other words, ambiguity initiated more complex processing. Taken together, the experiments of Mistler-Lachman, Bever et al. (1973) and MacKay (1966)

support the Exhaustive Computation rather than the Trial-and-Error Hypothesis. On the other hand, caution is indicated here because the sentence completion procedure does not seem to be the "cleanest" way to learn about comprehension. From the subject's point of view, this task demands a long sequence of integrative and productive operations. And the high latency times in these studies (about 6 sec) attest to the difficulty of the procedure.

A stricter test of the rival hypotheses, then, is provided by a task that taps the within-sentence, moment-by-moment demands of comprehension. Foss (1970) has used a "phoneme monitoring" task that appears to meet this criterion. In this procedure, the subject listens to a sentence and tries to comprehend it. At the same time, he is listening for a word beginning with a specific phoneme. On hearing such a word, he presses a button. The rationale of this task is that phoneme monitoring and comprehension ("semantic monitoring") draw on the same central "space." Therefore if comprehension temporarily requires more space, the phoneme task is displaced and reaction time should be slower. This task allowed Foss (1970) to ask whether or not an ambiguous input requires more space than a corresponding nonambiguous input. Sentences containing lexical and deep structure ambiguities were used. Presumably, the immediate context of the sentence did not favor either interpretation. Nonambiguous control sentences were constructed by making small changes in the ambiguous originals. For example, 11 was the nonambiguous equivalent of 10:

10. The new men started to DRILL <u>b</u>efore they were ordered to do so.
11. The new men started to MARCH <u>b</u>efore they were ordered to do so.

(Notice that in each case the critical phoneme closely follows the point of ambiguity.) Since the context following the ambiguous item does not resolve the ambiguity, the Trial-and-Error Hypothesis does not predict a reprocessing effect here (as it did in Foss et al. 1968 and Cairns 1973). But the Exhaustive Computation Hypothesis does predict slower reaction times for ambiguous sentences, since the subjects' "semantic monitoring" task is assumed to be more complex. Results supported the Exhaustive Computation view. Reaction times were significantly longer to phonemes in ambiguous than in nonambiguous sentences. Type of ambiguity had no effect. One possible drawback with this study, though, is that Foss apparently did not control for frequency. Latency times in this task are slower following infrequent than frequent items (Foss 1969), so the results may be due in part to this, IF the ambiguous words were much less frequent than their nonambiguous controls. (Unfortunately this cannot be readily determined since the materials used were not published.)

In spite of these possible flaws, on balance it seems fair to conclude that the Exhaustive Computation Hypothesis has better empirical support than the Trial-and-Error view. When a person hears an isolated ambiguous input in a neutral context, at some level of processing all meanings are considered, and this hampers his speed of comprehension. This explanation provides the best fit to the available data, but it raises another question: What happens when an ambiguous input is encountered not in a neutral context, but in a context that clearly favors one or the other interpretation? One possibility is that it is still necessary to exhaustively compute all potential meanings. Under this view, biased context does not reduce processing speed. It does, however, affect the CONTENT of the decision finally made since the interpretation chosen is the one that fits best with previous context. Alternatively, context could reduce the amount of processing necessary, perhaps by pre-selecting or "pre-activating" the appropriate meaning.

Two experiments have tested these hypotheses. The first, by Foss and Jenkins (1973), used the phoneme monitoring task described above. But in contrast to Foss (1970), in which the other parts of the sentence were appropriate to either interpretation, in this study a single word was altered to make one or the other meaning more probable (word frequency was controlled for this time). Foss and Jenkins used sentences like these:

12. The merchant put the STRAW beside the machine.
13. The farmer put the STRAW beside the machine.
14. The child put the STRAW beside the machine.

Given the context word merchant in 12, subjects were as likely to perceive the 'drinking-aid' as the 'hay' interpretation of straw. But when merchant was changed to farmer (13), subjects almost all perceived the 'hay' meaning; given child (14), the 'drinking' meaning was seen. For this sentence, then, merchant provided a "neutral" context, while farmer and child were "biased"contexts. Foss and Jenkins reasoned that if a biased context reduces the extra processing usually caused by ambiguity, sentences like 13 and 14 should have faster reaction times than those with a neutral context. On the other hand, if all meanings are processed regardless of context, no differences should be found. The results endorsed the latter view. Whether an ambiguous item followed a biased or neutral context, it made no (significant) difference to the reaction times: they were always slower than following the nonambiguous control word.

The other experiment examining context effects was conducted by Conrad (1974). She used a "color-naming" technique, which appears to be capable of identifying the meanings activated during comprehension. In this procedure (developed by Warren 1972), the subject

first listens to semantically-related words (e.g., robin, canary, sparrow). Then he is visually presented with a single word: this is either an item from the list (e.g., robin), a category name common to the list words (e.g., bird), or an unrelated word. The display word is printed in a colored ink; the subject's task is to name the color as quickly as possible. Warren (1972) found that color-naming reaction times were significantly longer to the repeated word and the category name than to the unrelated word; that is, an INTERFERENCE effect was found for these items. He concluded that during comprehension, category names for words are activated in memory as well as the words themselves. Conrad reasoned that any category name that does show an interference effect can be assumed to have been activated by the word in the list. It follows that this technique is useful for the study of ambiguity: by placing lexical ambiguities in neutral and biased contexts, one can determine whether BIASED contexts reduce the number of meanings activated. Conrad used sentences and colored display words such as these:

	SENTENCE	DISPLAY WORD
15.	We boiled the water in the pot.	Utensil (right category)
16.	The field was full of pot.	Utensil (wrong category)
17.	It is illegal to smoke pot.	Marijuana (right category)

Table 2. Sample of materials (from Conrad 1974)

These sentences all have biased contexts. The display word following each sentence represents either an appropriate (right) or inappropriate (wrong) category for the ambiguous item. If biased context "pre-selects" one meaning to be activated, reaction times should be slow only when a correct category is displayed; i.e., color-naming times will be slower following sentences 15 and 17 than 16. But if context does not affect the number of interpretations activated, there will be no differences in reaction time to the three sentences. (In effect, this means the 'utensil' interpretation of pot is activated even though the context clearly indicates the 'marijuana' meaning is intended.) Results supported the "context-insensitivity" view. Like Foss and Jenkins, Conrad took these findings as support for a model of comprehension in which all major meanings of a lexical item are initially activated. Context has its effect at some later stage of processing, and it affects only the content, not the speed, of comprehension. It remains to be seen, however, whether these conclusions would be supported given more extended contexts. Also, the conclusions of Foss and Conrad appear to be incompatible with those of Carey, Mehler, and Bever (1970), who showed that subjects induced to have a strong "set" for a particular syntactic structure could comprehend ambiguous sentences just as quickly as nonambiguous ones. At the present time, then, the interactions of context, set, and ambiguity remain unclear.

2. TWO MODELS OF COMPREHENSION

Let me review the ground covered so far. Psychologists have been interested in ambiguity because anything learned about how people understand ambiguous sentences may generalize to other aspects of comprehension. Psycholinguistic experiments with ambiguity have found evidence both for a Trial-and-Error and an Exhaustive Computation view of sentence processing. On balance, however, Exhaustive Computation seems to have stronger empirical support. The best evidence to date indicates that this is true even when the ambiguous unit is embedded in a context that clearly favors a particular interpretation.

This is all very well, but by staying so close to the data we have not yet caught a glimpse of the larger picture. That is, it is important to incorporate these findings into a more broadly-based theory of comprehension; specifically, a model of ambiguous language comprehension. What sort of model is possible, and what sort is best-suited to the purposes of psycholinguistics? There are many possible varieties, of course, but here I wish to distinguish two general classes, two approaches or "attitudes" to the comprehension problem. For convenience, these may be labeled "Dictionary Models" and "Network Models."

Dictionary Models view language comprehension as a process that involves specific, independent sub-processes. Each sub-process, in turn, may be associated with an isolable sub-system. One particularly important subsystem is a hypothetical "internal dictionary"; i.e., the information a person has about the meaning of words. I use the term Dictionary Models to refer to theories of comprehension that take this "subsystem" approach. The linguistic roots of Dictionary Models seem to lie in standard transformational theory (Chomsky 1965; Katz & Fodor 1963). This theory emphasizes the different roles played by independent components - syntactic, semantic, phonological - each of which represents a step in the comprehension sequence. Psychologists who have adopted this strategy have tended to focus attention on the internal dictionary, or SUBJECTIVE LEXICON, which is considered to be an isolable "place" in memory. Conrad, whose study of context effects in comprehension was discussed above, described the subjective lexicon as

> a set of concepts which have been acquired and refined over a relatively long period of time, a set of dictionary entries which define words, specify their selection restrictions, and perhaps provide information about their possible syntactic roles (1974:130).

Conrad argued for the existence of a subjective lexicon on the basis of her findings that: All major meanings of an ambiguous word are activated in a neutral context; all meanings are activated even when context is biased. She proposed that comprehension involves, first, a lexical look-up process that is independent of context, and in which all meanings are activated. Only at a later stage in comprehension does contextual information have an effect. ("Context" here refers both to the linguistic context of the sentence and discourse as well as to "nonlinguistic context": the environment and the listener's "knowledge of the world.") The effect of this context integration stage is to arrive at an interpretation that is consistent with what has preceded the input and consistent with what the person knows.

Linguistic dictionary models such as Katz and Fodor's have been criticized on the grounds that their distinction between dictionary knowledge and world (encyclopedia) knowledge is, at best, fragile (Bolinger 1965). Nevertheless, Katz (1972) has maintained the division. He now includes in the dictionary any information that increases the predictive power of the semantic component, while superfluous, factual information about the referents of words is contained in the encyclopedia. For instance, the knowlege that bachelors are 'human, male, adult, unmarried' goes into the dictionary; that 'bachelors are over an inch tall' is encyclopedia information.

Katz is concerned with semantic theory and not with psychology, so he can hardly be criticized for failing to provide a model of language performance. However, if psychologists incorporate his ideas into their performance models, "psychological reality" then becomes a pertinent issue. Dictionary Model psychologists presumably accept the distinction made by Katz between the dictionary and the encyclopedia. But for the purposes of psycholinguistics, is the separation of word and world knowledge really necessary?

Consider the concept 'bachelor'. When people think about bachelors, they may (rightly or wrongly) think of a six-foot tall fellow, surrounded by women and driving a sports car. That is, one of the tacit bits of information we use in talking and thinking about bachelors IS that they are more than an inch tall: if they weren't, how could they drive sports cars? Moreover, a piece of "superfluous," factual information may be ESSENTIAL for comprehension; e.g.,

18. Sam knows several bachelors who are poor high-jumpers: they can jump only three feet high.

In this sentence, the information 'over an inch tall' is more than tacit. In constructing an interpretation for this sentence, people probably use what they know about the average height of bachelors.

The knowledge is relevant, because three feet is "poor" for a high jump only if the jumper is not under, say, five feet in height. Admittedly, the information 'unmarried' is probably stored directly with the concept 'bachelor' while 'over an inch tall' may be "several concepts away." That is, the latter may be stored with 'person,' but it is retrievable because a bachelor is a man and a man is a person. The point is that for a psychological model there is no need to assume separate stores (dictionary and encyclopedia) - only different places in the same store. This could be thought of as an "information continuum" that stretches from more or less linguistic to more or less nonlinguistic knowledge. Chafe (1972:67) put this argument succinctly when he wrote:

> we do not use only part of what we know when we say something, we use all of it, and there is no way to divide knowledge that is linguistically relevant from knowledge that is not.

Dictionary Models of comprehension tend to emphasize the role of the subjective lexicon. At the same time, extralinguistic elements - context, socio-physical settings, expectancy, long-term memory and the like - tend to be ignored or downplayed. In a word, Dictionary Models give the impression that language understanding is solely linguistic rather than psychological AND linguistic. Thus, they fail on two counts. They do not REPRESENT the continuity of linguistic and nonlinguistic information, and they do not account for language PROCESSING as a joint product of cognitive and linguistic elements.

But is there an alternative to Dictionary Models? I believe an option is suggested in the work of some computer scientists and cognitive psychologists interested in modeling human memory (Anderson & Bower 1973; Collins & Quillian 1972; Quillian 1968, 1969; Rumelhart, Lindsay, & Norman 1972; Schank & Colby 1973). These investigators have found that "Network Models" are a satisfactory means of representing different types of information. Although an extensive review of this so-called "Zeitgeist in theoretical psychology" (Anderson & Bower 1973) is not possible here (see Frijda 1972), I do want to suggest that Network Models succeed in the two places Dictionary Models fail: they represent the continuity of linguistic and world knowledge, and they are "multifactor" theories of language processing. And just as Dictionary Models seem to correspond to transformational linguistics, Network approaches are congenial to some linguists, but that is an observation I will expand later.

Meanwhile, what are Network Models and how do they differ from Dictionary approaches? Perhaps the most important difference is that Network Models do not distinguish between "semantic" and "factual" information. Instead, ALL types of information - linguistic, cognitive, sensory - are contained in a single information network. And they are all represented in the same way; namely, in associative, or relational, form. Briefly, this associative structure may be illustrated as "aRb molecules," where a and b are atomic units and R the relation between them (Frijda 1972). In contrast to Dictionary Models' treatment of meaning (e.g., a set of propositional terms in a dictionary entry), in Network approaches the meaning of a particular concept is given by its location in a network of interconnected nodes; that is, by its relations with other units. Each of these other units is itself defined by its environment. Hence the "full concept" of a word is defined as all the nodes and all the relations that can be found by an exhaustive tracing process, originating at the initial concept (Quillian 1968:227). For instance, the full concept of bachelor would include its supersets, properties, and instances, as well as factual information about bachelors such as 'they are over an inch tall.' Furthermore, Network Models' characteristic mode of representation - the aRb associative format - represents not only concepts, but also "events" and "episodes" (see Norman 1973 or Rumelhart et al. 1972).

In contrast to Dictionary Models, then, Network approaches posit a single network of information in which every unit is ultimately connected to every other. In fact, this network is considered to be a simulation of human MEMORY. Understanding how memory is organized is important for psycholinguistics, because people use their memories, in general, "to cope with the world," and in particular, to produce and understand utterances. As Quillian (1968:221) said:

> Here it will be assumed that humans, in using language, draw upon and interact with the same memory in which their nonlinguistic information is stored. Under this assumption semantic memory is simply general memory and hence must be flexible enough to hold anything that can be stated in language, sensed in perception, or otherwise known and remembered. In particular, this includes facts and assertions as well as just objects and properties.

By viewing semantic memory as general memory, Network Models are in a position to illustrate somethingDictionary Models do not: the interdependence of memory and comprehension. Under this view, comprehension is possible only if a person can relate what he hears to information he has stored in memory (Collins & Quillian 1972). But, comprehension also affects memory, because comprehension is in

effect a command to add to or modify knowledge already stored. Suppose, as an example, that someone shouts at me: "Your house is on fire!" In understanding this utterance, perhaps I begin by "locating" in memory what I know about 'houses' and 'fires'. By relating the two areas of information, I construct a unique interpretation (and probably an image) of the statement. This interpretation is then added to general memory. That is, it modifies "what I know about the world," and therefore this information can be used in my future dealings with the world. So if I then call the fire department, it is partly because I was able to use memory to comprehend an utterance, and then store my interpretation of it in some useful form.

In short, comprehension involves more than a subjective lexicon of word meanings. An adequate model must also be able to specify the influence of context, setting, expectancy, and memory. That these ARE important becomes especially clear when one considers what is needed for a model of ambiguous sentence comprehension. How, for instance, do people comprehend

12. The merchant put the STRAW beside the machine.

Assuming the listener has two concepts with the name straw, a Network approach says that both concepts become activated. They are then EACH evaluated, one at a time, and the concept with the greatest activation-index is the "winner," that is, is perceived first (Collins & Quillian 1972). But what determines the amount of activation? This is assumed to be an idiosyncratic matter, so it is here that personal beliefs and biases play an important role. For instance, some people may assign an 'agricultural' interpretation to straw, because for them 'agriculture' and 'merchants' are closer semantically than 'agriculture' and 'drinking aids'. For comparable reasons, others may decide straw means 'drinking-straw'. Apart from the immediate context, agricultural concepts may be consistently perceived first by a person who grew up on a farm. In fact, subjects are about evenly divided in the meaning they see first in this sentence (Foss & Jenkins 1973). But when the context word was changed from merchant to farmer -

13. The farmer put the STRAW beside the machine -

the percentage of subjects seeing the agricultural meaning increased from 48 to 95%. One way to account for this increase is to say that the context pre-selected or pre-activated the "agricultural areas" of people's semantic memories. Nevertheless, the Exhaustive Computation process - activation and evaluation of all major meanings - is apparently still required in sentences of this type.

But surely this phenomenon has a simpler explanation: when a person

hears farmer, then straw, he decides the agricultural meaning of straw must be the right one, because farmers are more likely to be putting hay-straw than drinking-straws beside their machines! Although the person may be reasoning in this way at some level, the notion that a concept can be "pre-activated" is not quite as far-fetched as it may seem. Evidence for this has come from dichotic listening studies by Lackner and Garrett (1972) and MacKay (1973). MacKay's demonstration of the influence of context on semantic decision-making is the more convincing of the two. He had subjects repeat a sentence that was presented to one ear through a headphone, while ignoring inputs to the other ear. Unknown to the subjects, the sentences they were "shadowing" were ambiguous, and the word presented to the other ear (which they ignored and could not remember) represented one interpretation of each ambiguous sentence. An example is:

19a. They threw stones toward the BANK yesterday.
b. RIVER (unattended word)

MacKay found that unattended material significantly altered the number of subjects perceiving a particular interpretation in lexical and surface structure ambiguities (but not deep structure ones). Apparently, then, even context that is not consciously perceived can activate a concept. By extension, it seems reasonable to suppose that prior context that IS attended to can "pre-activate" a certain interpretation.

It was mentioned above that Network Models have been used mainly by psychologists and computer scientists, but that this approach has also been taken by some linguists. Although they are not the only ones to view language as a "network," the stratificational linguists are especially interesting from the standpoint of psycholinguistics. The correspondences between stratificational linguistics and Network Models of memory are clearer when one recalls that stratificational theory has been variously presented as "relational network theory" (Reich 1970) and "cognitive linguistics" (Lamb 1971). Like Network Models, stratificational theory uses a RELATIONAL format to represent a person's knowledge of the language. Language as a whole is considered to be a NETWORK that mediates between thought and sounds; this network consists of NODES, which are connected, by WIRES, to every other node. And it is through this relational network of nodes and wires that ACTIVATION is sent in language performance. A further similarity between stratificational theory and Network Models of memory is that both stress the continuous nature of linguistic and nonlinguistic knowledge. As Lamb made clear in his paper, "Linguistic and Cognitive Networks," language and cognition are not two "isolable" subcomponents, but rather are best considered "two parts of one large relational network" (1970:218).

The cognitive network may be thought of as a stratum in itself (the gnostemic stratum). Alternatively, it may be identified as the "conceptual system," and as such it contains

> all of the individual's knowledge of his culture, his personal history, his physical environment, in short everyting he knows, save the language itself, which is already represented in the lower strata (Lamb 1971:119).

Clearly, the "conceptual system" in Lamb's sense is equivalent to "general memory" in Quillian's (1968). This is what is used and modified in cognition generally, and in language use particularly.

3. CONCLUSION

Results from psychological experiments need to be incorporated into more comprehensive theories; otherwise, data are just data. But if two or more theories can fit the data, it seems important to spell out the assumptions and implications of each. In this paper, data from psycholinguistic studies of ambiguity were used to compare Dictionary and Network Models of comprehension. I suggested that Network Models provide a more satisfactory framework for psycholinguistics, since they can more easily represent the continuity between linguistic and cognitive information, and can show more readily why contextual and psychological elements are important in understanding language.

In their book, Human associative memory, Anderson and Bower (1973: 151) wrote:

> The most fundamental problem confronting cognitive psychology today is how to represent theoretically the knowledge that a person has: what are the primitive symbols or concepts, how are they related, how are they to be concatenated and constructed into larger knowledge-structures, and how is this 'information file' to be accessed, searched, and utilized in solving the mundane problems of daily living.

Although the statement above refers only to cognitive psychology, it seems that this is equally a problem for a cognitively-oriented linguistics. And ambiguity may be one of the phenomena that will help us determine how knowledge of all kinds is stored and used.

NOTES

[1] I am grateful to Rob Fink, Joanna Blake, and Laura Rice, who made many helpful suggestions on earlier versions of this paper.

[2] Address: Graduate Program in Psychology; York University
4700 Keele St., Downsview, Ontario M3J 1P3.

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IMPLICATIONS OF THE STUDY OF AMBIGUITY AND HOMONYMITY FOR GRAMMATICAL THEORY

Dale E. Woolley
University of Illinois at Chicago Circle

(1) Abstract

The notions of constructional homonymity and ambiguity are often evoked in evaluating grammatical theories. For instance, Wells (1947), attempting to provide constituent structure grammar with a theoretical foundation, examined ambiguity in such utterances as *old men and women*. The argument of Hockett (1954) that forms, linear order, constructions, and IC structure are primitives in grammatical theory rests crucially on the existence of ambiguity. Examination of sentences displaying constructional homonymity and ambiguity has also played an important role in the rejection of constituent structure grammars and the acceptance of transformational models by many linguists. See, for example, Chomsky (1957 and 1965) and Postal (1964 and 1967). More recently, McCawley (1967), has shown that some cases of ambiguity cannot be handled by a deep structure analysis, but derive from semantic features.

Many additional examples could be cited, but those just discussed will suffice to make the point: Linguists generally have assumed that, as a kind of minimum requirement, a grammar must be able to assign a phonological representation, a structural description, and a meaning to each sentence in a language. If a sentence displays constructional homonymity or is ambiguous, then in fact that sentence is really two or more sentences which just happen to have identical phonological representations. A grammatical theory must be able to assign such a sentence different representations at some level. If a proposed theory cannot, it must be rejected.

Interestingly, very little linguistic research into the nature of ambiguity itself has been carried out. Exceptions include Kooij (1971), Lehiste (1973), and Woolley (to appear).

This paper will report on a study into ambiguity and discuss some implications of that study for grammatical theory. Sentences with ambiguous constituents *old men and women*, *flying planes*, and *the shooting of the soldiers* were constructed and given to readers who were instructed to read these sentences attempting to convey one or the other of the meanings. Listeners then attempted to attach one or the other of the meanings to the readings. For all sentences tested scores were significantly better than chance. Furthermore sentences in which the ambiguous constituent contained a surface constituent boundary (e.g. *old men and women*) could be disambiguated about 90% of the time. All sentences could be disambiguated about 60% of the time.

The phonetic cues for disambiguation were examined. Analyses by ear and by instrument were carried out. The cues appear to be primarily rhythmic (durational or junctural) and intonational. For example, in *flying planes can be dangerous*, a sequence with close juncture after *flying* and with weaker stress and falling intonation on *planes* conveyed the meaning 'planes fly'. A sequence with open juncture after *flying* and with stronger stress and rising intonation on *planes* conveyed the meaning 'someone flies planes'. Instrumental analysis confirmed analysis by ear.

The results of this study imply that for grammatical theory a reinterpretation of the notions homonymity and ambiguity is indicated and that semantically based theories most adequately can handle ambiguity.

(2) Introduction

Linguists characteristically have relied heavily on the treatment of constructional homonymity and ambiguity[1] in rejecting or accepting grammatical theories. For instance, Wells (1947), attempting to provide constituent (phrase) structure grammar with a "unified and systematic" theoretical foundation in his important 1947 paper on immediate constituents, examined ambiguity. Among his arguments in support of a grammatical theory based on IC analysis, Wells included discussion of such utterances as *old men and women* and the now classic *the king of England's people*. "One of the prime functions of analysis into IC's," according to Wells, "is to reveal a formal difference correlated with the semantic one." (1947:93) The utterances cited above each have two different meanings. Associated with each meaning is a different IC analysis. For example:

(1a) (old men)(and women) means 'women and old men'
(1b) (old)(men and women) means 'old men and old women'

The argument of Hockett (1954) that forms, linear order, constructions, and hierarchical (IC) structure are (at least potentially) independent and must therefore be recognized as primitives in grammatical theory rests crucially on the existence of constructional homonymity, called ambiguity by Hockett. "Our conclusion, then, is that hierarchical structure is a 'primitive' just as are forms and order. The demonstration turns necessarily on two considerations: (1) there must be many utterances in which the hierarchical structure is unambiguous, to afford a frame of reference; (2) there must be also at least a few in which the hierarchical structure is ambiguous, since otherwise the hierarchical structure would in every case be determined by forms and order, and hence not a 'primitive'." (Hockett 1954:220)

Associated with the development of transformational-generative grammar has been much investigation into the adequacy of constituent (phrase) structure grammar. (For example see Chomsky (1957 and 1965) and Postal (1964 and 1967)). Examination of sentences displaying constructional homonymity and of putatively amgiguous utterances has played an important role in the rejection of constituent structure grammars and the acceptance of transformational models by many linguists. "We were able to show the inadequacy of linguistic structure that stopped short of phrase structure by exhibiting cases of ambiguity and similarity of understanding that were unexplained on lower levels. But it turns out that there is still a large residue of unexplained cases even after the level of phrase structure is established and applied to English. Analysis of these cases demonstrates the necessity for a still 'higher' level of transformational analysis..." (Chomsky 1957:87-88) An example of such a case is *they are flying planes*, in which the relationships *planes* as subject--*flying* as verb or *flying* as verb--*planes* as object hold. In the so-called standard transformational-generative theory of 1965, constructionally homonymous sentences are provided with two or more configurations in the deep structure.

Hockett (1961) bases part of his argument for accepting constructional grammar (grammar for the hearer) and rejecting transformational-generative

grammar on an analysis of the strategies hearers might use in disambiguating such phrases as *old men and women* and *a man eating fish* when they occur in contexts. "Phrase grammar (as commonly understood) has a number of known weaknesses, the recognition of which has been the main motive behind the development of transform grammars. Constructional grammar does not share these weaknesses. In the first place, it is possible (as we shall see presently) for constructional grammar to handle quite simply those phenomena that any reasonably manipulable phrase grammar best leaves for a transformational overlay... Finally, and specifically with reference to grammar for the hearer, the allocation of ambiguity to IC organization and to constructions, and the elimination of homophony at the word level, seem to yield a kind of realism that a phrase grammar, or a phrase-and-transform grammar, can deal with only in a more indirect and complex way." (Hockett 1961:230)

More recent research indicating inadequacies in the standard theory has also involved the analysis of ambiguity. The argument of McCawley (1967) against autonomous syntax is based in part on showing that the two senses of *John and Harry went to Cleveland* (i.e. travelling separately or together) cannot be handled correctly by deriving the sentence from two different deep structures, one with conjoined subjects, the other with conjoined S's. Rather the difference in meaning has to do directly with semantic features associated with the notion 'go'. The ambiguity in the sentence arises in semantic structure. In supporting the lexicalist hypothesis as more viable than generative semantics as an alternative to the standard theory, Chomsky (1972:149,50) discusses such sentences as *I almost killed John*, in which the different meanings have to do with whether or not John was even wounded. That is, one may almost kill John by shooting at him and missing, or by seriously wounding him.

Some linguists have suggested that certain ambiguities are resolved at a level above semantic structure, specifically in the social context in which the utterance occurs. Consider, for example, *How about a drink?*[2] One social context in which the occurrence of this utterance is highly probably is in a bar. But the utterance can be made by either the bartender or a patron. In the first case its meaning is roughly 'would you like a drink' or 'may I serve you a drink', in the second, 'may I have a drink' or 'would you serve me a drink.' To assign different meanings to certain constructionally homonymous sentences it may be necessary for a grammatical theory to have access to social context.

Many additional examples could be cited (see Kooij (1971) for a fuller treatment), but those just discussed will suffice to make the following points. Linguists generally have (implicitly or explicitly) assumed that, as a kind of minimum requirement, a grammar must be able to assign a phonological representation, a structural description, and a meaning to each sentence in a language. If a sentence displays constructional homonymity or is ambiguous, then it really is two or more sentences with the same phonological representation. A grammatical theory must be able to assign such sentences different representations at some level. If a proposed theory cannot, it must be rejected.

Linguists typically appeal to their own intuitions in asserting that an example is or is not ambiguous. Readers must then accept these intuitions if they are to follow the line of argument. Very little work has been concerned with the nature of ambiguity itself. The purpose of the present study is to explore a fundamental question associated with ambiguity and to make some tentative suggestions for grammatical theory.

The fundamental question is whether sentences claimed to be ambiguous on paper can be shown to be ambiguous in speech, and, if so, to what extent. A correlative issue concerns the ways in which speakers and listeners may disambiguate sentences. This paper presents a report of an investigation into the nature of putative ambiguous sentences in speech. If listeners are unable to distinguish among the meanings of such sentences, then these sentences are in fact ambiguous and we have cases of constructional homonymity. If, on the other hand, speakers can consistently assign meanings to such sentences, then they are in fact not ambiguous. Suppose that we found that some aspect of the suprasegmental pattern varied systematically and persistently with the different meanings of reputedly ambiguous sentences. Such sentences would then *not* be homonymous, and a grammar would have to be constrained to assign them unique phonological representations. If, however, we find no systematic phonetic variations in suprasegmental patterns serving to distinguish the meanings of ambiguous sentences, then we might want to consider such sentences as constructionally homonymous, but not ambiguous. This is to say that speakers might learn a 'bag of suprasegmental tricks' which can help to resolve ambiguity.

(3) Procedure

The stimulus materials were three sentences suggested by examples of constructional homonymity discussed in Chomsky's *Syntactic Structures*: *old men and women*, *flying planes*, and *the shooting of the soldiers*. These nominals are ordinarily considered to be constructionally homonymous. Each develops through transformation from two different deep structures. *Old men and women* may be derived from *old men and women* (in general) or from *old men and old women*. *Flying planes* may be derived from *planes fly* or from *someone flies planes*. *The shooting of the soldiers* may be derived from *someone shoots the soldiers* or from *the soldiers shoot* (weapons).[3] As a result, each of these nominals is claimed to be also ambiguous. These nominals were used as the basis for constructing three sentences to be used as stimulus items for this study. These were *there are only old men and women here today*, *flying planes can be dangerous*, and *the shooting of the soldiers bothered me*. As explained above, each of these sentences has two meanings. In actual speech the particular meaning intended by a speaker would usually be unambiguous because of its context. In this investigation the sentences were studied in isolation.

Recording procedures

The speakers were three male and three female students from speech classes at the University of Illinois. They were judged by the experimenter to be superior speakers of General American English. Their mean age was 22 years.

The experimental session began with a period of instruction in which the ambiguities in the three sentences were explained. All six speakers reported that they readily understood the ambiguities. The speakers were informed that they would be asked to read each sentence twice, attempting to convey each meaning once. Each speaker was given six cards. Each card had written on it a number, one of the three sentences, and the meaning of that sentence to be conveyed. The speakers then were sent to isolated rooms where they were told to practice reading the sentences. Card numbers were called in random order and the speaker with that number reported and read the appropriate sentence attempting to convey the meaning indicated on the card. This process was continued until all 36 sentences had been read.[4] Speakers were not allowed to listen to one another's readings.[5]

Recordings of the readings were made on M-90 equipment in the sound-treated room at the Speech Research Laboratory at the University of Illinois.

Judging procedures

In preparation for the judging procedure, the individual sentence readings were cut from the tape recording, randomized, and spliced together. A judging sheet was designed that consisted of three columns. The center column listed sentence numbers, the right and left hand columns corresponded to the two possible meanings of each sentence. To correct for direction bias, the right and left hand columns were interchanged on half the judging sheets. The design of the judging sheets forced the listeners to make decisions on each sentence they heard. Furthermore, they were instructed to guess if they were unable to come to a decision. The listeners were 34 students, all native speakers of General American English, in the introductory phonetics course in the Speech Department at the University of Illinois.

The judging was done in the same room in which the recordings were made by groups of 8 to 12 listeners each. The recordings were played back over the same equipment on which they had been made. Transmission was through air via a speaker system. The ambiguity of each sentence was explained to the listeners in an instructional session before the readings were heard. The listeners also reported that they easily understood the ambiguities.

(4) Results

Table 1 displays the composite results of the experiment. Each subject gave two readings of each of three sentence types, attempting to convey one meaning in the first reading and the other meaning in the second reading. For convenience the meanings for each sentence type were divided into A and B sets as can be seen in the note at the bottom of Table 1. Thus a total of 36 readings were divided into six sentence sets according to meaning, each set consisting of six readings. For each reading 34 listeners checked one of two possible meanings for a total of 204 responses per sentence set. The second and third columns in Table 1 list the total number of A and B responses and the percentage of A and B responses for each sentence set. The total number of responses for intended meaning A for *old men*[6] is 203 as one judgment was missing.

It will be seen that for each sentence set the percentage of judgments corresponding to the reader's intention is greater than 50 percent or chance probability in a binary judging design. Moreover, in the fifth column it can be seen that the percentage of judgments surpasses chance significantly for all sentence types. Although no test for the significance of differences between sentence types was made, the data suggest that the *old men* type differs from the other two types and that distinctions of meaning are more easily produced and recognized for this type. The *soldiers* type shows slightly greater values than the *flying planes* type, but the difference between them is small. These facts will be more thoroughly discussed later in the paper.

Table 2 lists the total number of A and B responses for each reading. The percentage of correct responses or *score* out of the total number for each sentence is also presented.[7] Individual readings vary from 14.7 to 97.0 with respect to the percentage of correct responses. Six readings were judged to have meanings not corresponding to the readers' intentions. These are the readings which show a score of less than 50. For instance, when speaker DH

TABLE 1

Total number and percent of responses for each sentence set shown by type of response

Sentence type	A Res N	A Res %	B Res N	B Res %	Overall N	Overall %	X^2	df = 1
THERE ARE ONLY OLD MEN AND WOMEN HERE TODAY					325	79.8	157.4	p<.001
Meaning A intended 'women and old men'	143	70.4	60	29.6				
Meaning B intended 'old men and old women'	22	10.8	182	89.2				
FLYING PLANES CAN BE DANGEROUS					250	61.3	20.8	p<.001
Meaning A intended 'planes fly'	121	59.3	83	40.7				
Meaning B intended 'someone flies planes'	75	36.8	129	63.2				
THE SHOOTING OF THE SOLDIERS BOTHERED ME					262	64.2	32.9	p<.001
Meaning A intended 'soldiers shot (weapons)'	134	65.7	70	34.3				
Meaning B intended 'someone shot the soldiers'	76	37.3	128	62.7				

TABLE 2

Total number by type and score (% correct) of responses for each sentence read

Sentences	A Response	B Response	Score
THERE ARE ONLY OLD MEN AND WOMEN HERE TODAY			
Meaning A intended 'women and old men'			
DH	5	29	14.7
JM	15	18	45.5
LP	33	1	97.0
LW	33	1	97.0
MN	27	7	79.4
RT	30	4	88.2
Meaning B intended 'old men and old women'			
DH	2	32	94.1
JM	3	31	91.2
LP	3	31	91.2
LW	5	29	85.3
MN	4	30	88.2
RT	5	29	85.3
FLYING PLANES CAN BE DANGEROUS			
Meaning A intended 'planes fly'			
DH	24	10	70.6
JM	21	13	61.7
LP	21	13	61.7
LW	25	9	73.6
MN	12	22	35.3
RT	18	16	52.9
Meaning B intended 'someone flies planes'			
DH	14	20	58.7
JM	14	20	58.7
LP	9	25	73.6
LW	12	22	64.7
MN	10	24	70.6
RT	16	18	52.9
THE SHOOTING OF THE SOLDIERS BOTHERED ME			
Meaning A intended 'soldiers shot (weapons)'			
DH	28	6	82.4
JM	11	23	32.4
LP	29	5	85.3
LW	14	20	41.3
MN	30	4	88.2
RT	22	12	64.7
Meaning B intended 'someone shot the soldiers'			
DH	11	23	67.6
JM	15	19	55.8
LP	11	23	67.6
LW	11	23	67.6
MN	20	14	41.3
RT	8	26	76.5

tried to convey meaning A for the *old men* sentence, (14.7% correct judgments), his failure did not result in ambiguity. Rather, 85% of the listeners extracted meaning B from his reading. Another interesting observation to be seen in this table is that only a few sentences show correct judgments near 50 percent or chance probability. Only 8 out of 36 readings occur with between 40 and 60 percent correct responses. If these were in fact ambiguous sentences, we would expect many readings to occur with close to 50 percent correct responses. It would appear then that listeners are able to assign some consistent meaning to these ambiguous sentences when they hear them in isolation, although this meaning may not always correspond to the reader's intention.

Table 3 presents for each subject the total number of correct and incorrect responses for each sentence type, the overall percent correct, and the ratio of the number of correct to the number of incorrect responses as a sort of effectiveness index. The main thing to be seen in this table is that speakers vary in their ability to convey meanings of ambiguous sentences. Speaker LP was consistently effective with relatively high C/I for each sentence type, while speakers MN and JM were consistently ineffective with relatively low C/I for each sentence type. The other speakers varied unsystematically in effectiveness. Five speakers were successful in conveying intended meanings in over 64% of the cases.

(5) Discussion of results

At this point in the discussion it would be profitable to compare the results of this study with those recently reported by Lehiste (1973).[8] Lehiste's results are based on judgments made by thirty listeners (fifteen linguists and fifteen nonlinguists) of multiple readings of fifteen ambiguous sentences made by four speakers (two linguists and two nonlinguists) for a total of 184 productions. Lehiste's experimental design was similar to that employed in this study. Speakers attempted to convey the different meanings of ambiguous sentences and listeners assigned meanings to the readings. The linguists did not behave significantly different from the nonlinguists as either speakers or listeners. For ten of the fifteen sentence types listeners assigned meanings which corresponded to those intended by the speakers with significantly greater than chance probability.

Three of Lehiste's sentences are directly comparable with those used in this study: *the old men and women stayed at home*; *visiting relatives can be a nuisance*; *the shooting of the hunters was terrible*. The overall percent correct scores for the *old men* types are quite similar (Lehiste 80.6%, this study 79.8%). But the results reported by Lehiste for the other two types are considerably lower than those reported here (Lehiste 52.5% and 49.4%, this study 61.3% and 64.2%). The score for meaning B ('to visit relatives') of Lehiste's *visiting relatives* is, however, 65%. The differences in these scores are attributable at least in part to the generally higher effectiveness of the speakers in this study. The overall percent correct scores for Lehiste's speakers ranged from 56% to 70%, those for the speakers in this study from 58% to 79%. Another indication that the speakers in this study were more effective at disambiguation is the number of readings with near random scores. Only 22% of the readings here have scores between 40% and 60%, whereas 33% of Lehiste's readings fall in that range. Another interesting observation can be made about the *old men* types. In both studies the individual readings in these types include one sentence with random scores and one sentence in which the listeners unambiguously judged the

TABLE 3

Total number and percent of correct and incorrect responses and ratio of number of correct to number of incorrect responses by type of sentence and speaker

	Correct	Incorrect	% C	Ratio C/I
Speaker DH				
There are only old men and women here today	37	31		1.2
Flying planes can be dangerous	44	24		1.8
The shooting of the soldiers bothered me	51	17		3.0
Total	132	72	64.7	1.8
Speaker JM				
There are only old men and women here today	46	21		2.2
Flying planes can be dangerous	41	27		1.5
The shooting of the soldiers bothered me	30	38		0.8
Total	117	86	57.6	1.4
Speaker LP				
There are only old men and women here today	64	4		16.0
Flying planes can be dangerous	46	22		2.1
The shooting of the soldiers bothered me	52	16		3.3
Total	162	42	79.4	3.9
Speaker LW				
There are only old men and women here today	62	6		10.3
Flying planes can be dangerous	47	21		2.3
The shooting of the soldiers bothered me	37	31		1.2
Total	146	58	71.6	2.5
Speaker MN				
There are only old men and women here today	57	11		5.2
Flying planes can be dangerous	36	32		1.1
The shooting of the soldiers bothered me	44	24		1.8
Total	137	67	67.1	2.0
Speaker RT				
There are only old men and women here today	59	9		6.6
Flying planes can be dangerous	36	32		1.1
The shooting of the soldiers bothered me	48	20		2.4
Total	143	61	70.0	2.3

sentence to have the meaning the speaker did *not* intend. If these latter sentences are treated as having the alternate meaning, the scores for the *old men* types are higher, 86.8% for this study and 88.8% for Lehiste's study.

One problem associated with a study of this kind concerns the probability of occurrence of the possible meanings of an ambiguous sequence, as Lehiste notes. This difficulty is, however, not as serious as one might suppose. An examination of the *distribution* of scores over each meaning of the sequence will reveal those cases where likelihood of occurrence may have had an effect. In such cases the scores will be skewed and must be interpreted with care. Of Lehiste's eight sentences with significant scores (p .025), six have skewed distributions. The *old men* type reported in this study has a skewed distribution, but the score for each meaning computed separately is significant in any case. Lehiste's *old men* type has a skewed distribution because one reading scored zero. That reading really belongs to the alternate set of meanings. Both meanings of Lehiste's *I know more beautiful women than Mary* have high scores (above 60%). It is undoubtedly true that the meaning 'greater number of beautiful women' is more likely than 'women more beautiful than Mary' for this sentence, but the high scores still indicate disambiguation. Four of eleven sentences then have scores which are dubious: *the hostess greeted the girl with a smile* ('hostess smiled' 81%, 'girl smiled' 53%); *the police stopped fighting after dark* ('police quit fighting' 79%, 'police stopped the fighters' 51%); *the president of the university's committee came* ('president came' 85%, 'committee came' 47%); *the lamb is too hot to eat* ('animal is uncomfortable' 53%, 'roast is steaming' 61%). When Lehiste's speakers, who had first read all the sentences without specific instructions, reported which meanings they might have had in mind, in 13 of 16 cases for these sentences they reported the meanings which show the higher scores.

Two of these sentences deserve special comment. Concerning the *president* sentence Lehiste says that since "the speakers did not agree among themselves about which of the two meanings is more common...the difference in scores cannot be immediately attributable to a relatively greater plausibility of meaning A (i.e. 'president came')." That the speakers, however, all had meaning A in mind for their initial reading and before they were asked to consciously consider the ambiguity indicates that meaning A is more probable. Lehiste is correct, I think, in noting that the length of the constituent associated with the second meaning presents difficulty in encoding. That constituent is also perhaps too long for memory span associated with decoding constituent structure. That the *lamb* sentence received a significant score is certainly due to the greater probability of the second meaning. The score of 61% for that meaning just manages to make the overall score significant. It is doubtful that this sentence can be disambiguated.

(6) Conclusions

The main conclusion to be drawn from this study is that when speakers attempt to attach a particular meaning to a constructionally homonymous sentence (that is, when they consciously try to disambiguate), listeners can assign this same meaning to the sentence with significantly greater than chance probability. Some speakers are more effective in providing the cues for disambiguation than others. It is reasonable to assume that some listeners are also better than others in responding to these cues. This is one direction in which further research could be profitably aimed. Lehiste found that her fifteen sentences fell roughly into two groups, those which could be disambiguated and those which

could not. This division correlated with surface constituent structure. Homonymous sequences with two or more unlabeled constituent structures could be disambiguated; those with only one could not. Thus Lehiste found that *visiting relatives* and *the shooting of the hunters* could not be disambiguated. The results reported here show that *flying planes* and *the shooting of the soldiers* were disambiguated. Since this major finding is not supported by Lehiste's data, it should be discussed in detail.

In order to offer an explanation for this difference it is necessary to comment on strategies of disambiguation. Whenever listeners can consistently disambiguate, there must be phonetic cues present in the utterance. Lehiste's study shows that, at least for sentences with two or more surface constituent structures, timing relationships over the constituent boundary provide the cues. The boundary itself often has phonetic realization as pause, ritardando, or laryngealization. The *old men* sentences used here were disambiguated in just that fashion. If, on the other hand, sequences without a constituent boundary are to be disambiguated, the speaker must somehow supply those sequences with cues. Can he do so, and, if so, how?

Analysis by ear and examination of narrow band spectrograms of readings indicates that the *flying planes* sentences were most successfully disambiguated when timing, stress, and intonation factors were all involved. A sequence with close juncture,[9] and weaker stress and falling intonation on *planes* (flying=planes) was identified as A 'planes fly,' i.e. the subject-verb relationship. A sequence with open juncture, and stronger stress and rising or rising-falling intonation on *planes* (flying+planes or planes) was identified as B 'someone flies planes,' i.e. the verb-object relationship. Actually *flying* always occurred with an intonation contour, usually rising-falling, but this fact does not seem crucial. In one successful verb-object reading (MN 70.6%), the speaker had a glottal stop between the words. Less successful readings tended to have ambiguous cues, that is, a mixture of cues associated with each meaning. For instance one intended A reading (JM) occurred as flying+planes and was identified 61.7% A. Here the stress-junctural and intonation patterns conflict, but the intonation pattern prevails. The one reversal reading in this set (MN intending A) occurred as flying+planes with extra-heavy stress on planes and received a score of 35.3%. In other words it was identified as a B reading.

For *the shooting of the soldiers* a sequence with rising-falling intonation on *shooting*, open juncture after *shooting*, and weaker stress and falling intonation on *soldiers* (shooting+of=the=soldiers) was identified as A 'soldiers shoot,' i.e. the subject-verb relationship. One of the best A readings (MN 88.2%) has extra heavy stress and a clearly audible intonation contour on *shooting* followed by a period of laryngealization. Sequences identified as B 'shoot soldiers,' i.e. the verb-object relationship showed less uniformity. *Soldiers* may have either the combinations weaker stress and falling intonation or stronger stress and rising intonation. What the successful B readings have in common, however, is close juncture after *shooting*. The two readings by JM and intended reading B by LW offer interesting comparisons. Intended reading A by JM and intended reading B by LW are as nearly alike in timing, stress, and intonation as the ear can detect. Narrow band spectrograms of the two readings are nearly identical. Both readings have primary stress on *shooting*, secondary stress on *soldiers*, and an intonation contour that rises on *shooting* and then gradually falls over the entire phrase. The timing relationships are peculiar, the entire sequence seemingly stretched in time. LW's reading scores 67.6% B, JM's scores 32.4% A (i.e.

67.6% B). The two readings of JM are alike in stress and intonation and different only in that the timing in intended reading B is not unusual. Both are identified as B readings.

Clearly speakers are able to employ timing, stress, and intonation to *label* constructions. In the case of *flying planes* the participial (subject-verb) relationship is labeled by providing the constituents with the suprasegmental pattern of a compound noun; the gerund (verb-object) relationship is labeled by providing the constituents with the pattern of verb-object or attribute-head, the pattern predicted by the bracketing. (Chomsky and Halle, 1968) Put another way, the participle is marked, the gerund unmarked. Lehiste reports a similar situation with *German teachers visit Greensboro* in which the meaning 'teachers of German' has the (marked) pattern of a compound noun and the meaning 'teachers from Germany' has the attribute-head (unmarked) pattern. In the case of *the shooting of the soldiers*, both the subject-verb relationship and the verb-object relationship appear to be marked in contrast to a third pattern predicted by the bracketing convention.[10]

Once the patterns which yield the most successful readings have been identified, it is possible to use that information to predict for a particular reading what interpretation listeners should assign it. For the 24 readings in this study one can make nineteen predictions. No prediction can be made when the cues are ambiguous, the situation in five cases. Fourteen of the predictions are confirmed by the listeners' interpretations. The recordings of *visiting relatives* and *the shooting of the hunters* sentences of Lehiste were examined. No predictions can be made in eleven of 24 cases. One prediction is unconfirmed.

The explanation for the difference between Lehiste's results and those of this study is, therefore, attributable to the skill of the speakers in assigning suprasegmental labels to constructions. Since speakers differ in their ability to disambiguate in general, it is reasonable to expect them to differ in their labeling skills. Lehiste's speakers failed to provide cues as often as the speakers in this study did. Whereas Lehiste identifies her speakers as two linguists and two nonlinguists, the speakers used here were experienced and thus might be expected to perform better.

(7) Implications for grammatical theory

In discussing the implications of this study for grammatical theory, it will be useful to comment more thoroughly on the terms *constructional homonymity* and *ambiguity*. Although the terms are often used synonymously, here an attempt has been made to keep them separate. If at some point in the grammar a sequence is assigned two or more different representations which are subsequently neutralized so that only one phonological representation[11] results, that sequence is constructionally homonymous. If a sequence has two or more meanings, it is ambiguous. The relationship between homonymity and ambiguity will be defined by grammatical theory. Because these terms are often used synonymously, an equivalence relationship in grammar might be an attractive possibility. Since sequences such as *flying planes* can be disambiguated, the grammar could assign them suprasegmental labeling of the sort discussed above. They would then not be constructionally homonymous. Actually a sort of biuniqueness condition on syntax-semantics would be imposed on a grammar which attempted to equate homonymity and ambiguity. Two meanings would imply two structures; two structures

would imply two meanings. The rather unfortunate consequence of this condition is that the one-to-one mapping would rule out constructional homonymity and ambiguity from linguistic theory entirely.

In point of fact sequences such as those discussed in this paper may or may not be ambiguous on a given reading. Consequently, constructional homonymity and ambiguity are not equivalent. The grammar will apparently need to treat some of these sequences as constructionally homonymous, and some as not. For instance sequences which can be assigned two or more different surface constituent structures will not be treated as homonymous, although a given reading of such a sequence may be ambiguous. But what sequences will be treated as constructionally homonymous? The implication in Lehiste's study is that these would be ambiguous sequences with only one unlabeled surface constituent structure. But the results reported here suggest that greater restrictions obtain. That speakers can disambiguate some sequences by providing them with a sort of suprasegmental labeling does not mean that all such sequences can be disambiguated. It is hard to imagine that an ambiguity arising from homophonous lexical items (e.g. *bear*) could be phonetically disambiguated. And, when a sequence is suprasegmentally labeled, do speakers tend to provide it with identical cues? The results of this study are inconclusive on this point. Speakers did not employ identical cues for disambiguation, but their strategies for disambiguation were remarkably similar. One solution is to regard the suprasegmental labeling as systematic, thus accounting for the fact that disambiguation can occur, and to regard the specific cues for disambiguation employed by speakers as phonetic realizations of that labeling in free variation.

Furthermore the results of this study call into question another commonly held belief: the distinction between *competence* and *performance*. As ordinarily understood, competence has to do with a speaker's intuitive and largely unconscious knowledge of the grammar of his language. Performance has to do with the way in which that knowledge is put to work in concrete speech situations. The competence of speakers is generally thought to be vastly greater and more complex than the facts of any particular performance indicate. So it is within a speaker's competence to assign two structures (meanings) to a sequence such as *flying planes*, but not within his performance to convey the distinctions implied by those structures in speech. But the results of this study show that it *is* within a speaker's performance capabilities to do just that.

Finally the study of ambiguity and constructional homonymity provides support for a theory of language such as that proposed by Lamb (1966), which recognizes differently identifiable levels of structure. Constructional homonymity and ambiguity may apparently arise at any linguistic level. There are utterances such as *how about a drink*, discussed earlier, in which the ambiguity is associated with the social context in which the utterance is produced. Such utterances are socially ambiguous.

It is often difficult to decide for a particular sequence whether constructional homonymity and/or ambiguity is semantic or syntactic. Until grammatical theory has successfully dealt with the relationship between syntax and semantics, the examples discussed in this paper will have to be considered either semantically or syntactically homonymous. A few observations, however, can be made. Constructional homonymity at the syntactic-semantic level is often associated with underlying and surface constituent structure, word order, and word class. *Flying planes* has two underlying, but only one surface constituent structure.

Old men and women has two underlying and surface constituent structures. In addition to constituent structure, word order may also play a part. *He rolled over the carpet* is constructionally homonymous, but *he rolled the carpet over* is not. An example cited by Francis (1958:285) involves word class. In *the child looks backward*, *backward* may be either adjective or adverb.

That constructional homonymity and ambiguity can arise at the lexical level is so obvious that discussion is unnecessary. It is simply a fact about language that a word may have different meanings and that different words may have the same phonological representation. To use an example from Katz and Postal (1964:15 and 25), *he enjoys wearing a light suit in the summer* is ambiguous because *light* has more than one meaning, and *the bank is the scene of the crime* is ambiguous because *bank* is the surface manifestation of at least three different words.[12] Also idioms are frequently ambiguous. Makkai (1969 and 1972) has shown that idioms may arise at either the semantic or lexemic level.

Morphological homonymity is apparently rarer, but seems to be of two types. It sometimes happens that different morphemes have the same phonological realization. *The friends here are the girls* is three-ways ambiguous because the inflectional suffix *s* of *girls* may represent the plural, the singular possessive, or the plural possessive.[13] More often general phonological processes such as assimilation, metathesis, and neutralization, or specific rules map different underlying morphological representations onto the same surface structure. *Fissure* and *fisher* have the same pronunciation, but *fissure* has underlying *s* (cf. *fissle*) and *fisher* underlying *š*.[14] Neutralization (especially of the voicing distinction word finally) is a rather common source of homonymity in a number of languages.

Homonymity may arise at the phonological level itself, often associated with sandhi and junctural phenomena in general. Of many examples that could be cited consider the old standby *a name/an aim*. Each has a different phonological representation, but ambiguous readings are possible, since the phonological representations can be neutralized phonetically. Another nice example is *do you know Joyce is dead/Joyce's 'Dead'?*, i.e. the story by James Joyce. Most instances of phonemic overlap (Bloch 1941) are in fact cases of morphological homonymity. Some, however, qualify as phonological homonymity. For many speakers both *prince* and *prints* have [nts], but *princess* has [ns]. These facts could, of course, be handled by an insertion rule operating on an underlying representation.[15] But phonetic processes are in question here and should be treated at the phonetic level (cf. Woolley 1970).

Constructional homonymity, then, can arise at any level in linguistic structure. Some constructionally homonymous sequences will be ambiguous, others will not be. If a sequence is unambiguous, phonetic cues must be available to the listener. In some instances these cues will be predictable; that is, they will be phonetic realizations in the form of timing, stress, and intonational relationships of markers assigned to the sequence in the syntax-semantics. *Old men and women* is a sequence of this type. In other instances, it is reasonable to imagine, the cues will not be predictable by the grammar; that is, they will be supplied from an inventory of phonetic modifications available to speakers and listeners alike to increase the likelihood of being understood. As a result, these cues will differ from reading to reading, and speakers and listeners will differ in their ability to use them. Speakers, of course, can

always choose an unambiguous paraphrase, and will ordinarily do so. The point is that they do not *have* to do so in every case.

Perhaps more interesting for grammatical theory are sequences which are not constructionally homonymous, but yet are ambiguous. Most cases arise accidentally at the phonetic level because of fortuitous timing-stress-intonation relationships in a specific reading. So, for instance, in Chicago speech, if one gives his address as '2950 North Western Avenue,' a listener unfamiliar with Chicago is likely to ask how to get to Northwestern Avenue. But as Loesch (1965:261) points out, it is possible to give sequences such as *I'll move on Saturday*, which are ordinarily unambiguous, ambiguous readings. In other words speakers can choose to *erase* predictable phonetic cues. There must then be direct links between the phonetic level, where the cues do or do not occur, and some higher level of abstraction, where the choice to ambiguate or disambiguate is made. Lamb's stratificational model provides such links.

Footnotes

[1]In this paper 'constructional homonymity' is a term that will be restricted to discussions of grammatical analysis. A sequence is constructionally homonymous if it has a single, unique phonological representation, but two or more representations at some higher level of analysis. A sequence is ambiguous if it has two or more meanings. The two terms are often used interchangeably. Here 'constructional homonymity' implies grammatical analysis. 'Ambiguity' implies (potential) utterance and perception of utterance. The relationship between constructional homonymity and ambiguity will be discussed more thoroughly later.

[2]I am indebted to Adam Makkai for bringing this example to my attention.

[3]The derivations of these sentences in a generative grammar are more complex. This simplification gets at the ambiguity in meaning and is sufficient for our purposes.

[4]Six speakers read three sentences two times each for a total of 36 sentences.

[5]For the remainder of the discussion *readings* will refer to the 36 recorded utterances, *sentence types* to the three ambiguous sentences, and *sentence sets* to the six meanings, two for each sentence type and hereafter designated A and B within each type.

[6]Hereafter the sentence types will be abbreviated *old men*, *flying planes*, and *soldiers*.

[7]Here a correct response is one which corresponds to the reader's intention.

[8]I wish to thank Professor Ilse Lehiste for kindly sending me her data and tape recordings.

[9]'Juncture', as employed here, refers to more than boundary phenomena. Timing over several segments is involved.

[10]A consequence is that ambiguous interpretations of *the shooting of the soldiers* should result from readings in which the unmarked pattern occurs, whereas ambiguous interpretations of *flying planes* should result from readings with ambiguous cues (cf. above). Furthermore readings of *flying planes* with the unmarked pattern should be interpretable! There is some indication in the data to support this contention, but this question and many others raised by this study can only be answered by studies involving speech synthesis. See Nash (1970) for one effort in this direction.

[11]More properly, perhaps, *phonetic* representation. See below.

[12]Katz and Postal consider *bank* to have three different meanings. These different 'meanings' of *bank* are so unrelated that *bank* is best considered a homophone.

[13]The sentence is, of course, not orthographically ambiguous, but that fact is irrelevant here.

[14]One might argue that this example is unconvincing because *fisher* has an internal morpheme boundary. The proper name *Fischer* would do just as well.

[15]The facts could also be handled by a deletion rule, but that treatment would require that *prince* and *prints* have the same underlying form.

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VISIBLE DISTINCTIVE FEATURES*

Peter A. Reich
University of Toronto

Abstract

Fingerspelling is a method of communication used in a number of schools for the deaf, in which each letter of the alphabet is represented by a particular hand configuration. Experienced users can communicate by this method at nearly the rate of normal speech. Careful analysis of this rapid hand movement shows a distinctive feature system similar to phonological distinctive features. Insights derived from fingerspelling suggest that there may be a better way to define phonological features than any of the ways currently being used. In particular, all current theories are founded on the notion that phonemes consist of simultaneous bundles of features of unit duration. However, the phonology appears to be greatly simplified, and also more in keeping with phonetic and neuroanatomical facts if phonemes are considered to consist of sequential instantaneous onset features. Implications for the phonology of English are also discussed.

Fingerspelling and the Rochester Method

The majority of people who are congenitally profoundly deaf learn some form of manual communication, which they use to communicate with their deaf friends and their families. There are two forms of manual communication — sign language and fingerspelling. As a result of the Gardners (1969) success with Washoe the Chimp, most linguists are now aware of sign language, which appears to be a morpheme-based system with a structure all its own. Another form of manual communication is fingerspelling, in which each letter of the alphabet is represented by a particular hand configuration (and movement, in some cases). People using this form of communication are supposed to spell out on their hands every letter of every word that would be spoken. Some schools for the profoundly deaf have their teachers simultaneously speak and fingerspell for all communication in the classroom. This simultaneous method is known as the Rochester method, or Visible English. It is used at schools for the deaf in Ontario, Louisiana, Tennessee, Florida, and New York. Experienced teachers can communicate via this method at a very rapid pace, although not up to normal speaking rate. The hand configurations of the letters of the alphabet used in North America is given in Figure 1. Other alphabets in widespread use are the British two-handed alphabet, and one in use in the Soviet Union (used by all Soviet schools for the deaf).

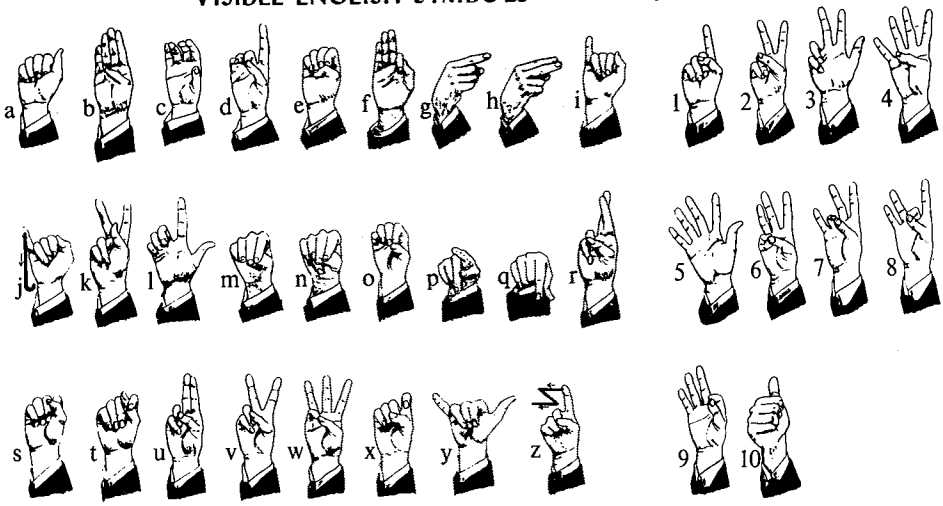
Acquisition of Fingerspelling

Hearing adults in contact with deaf children are taught the individual letter configurations and are told to produce them fluently while simultaneously speaking the word normally (Guillory, 1966). While all the letters of a word are supposed to be produced individually in sequence, the words are supposed to be read as whole units. This is like typing, in which each letter is typed, but

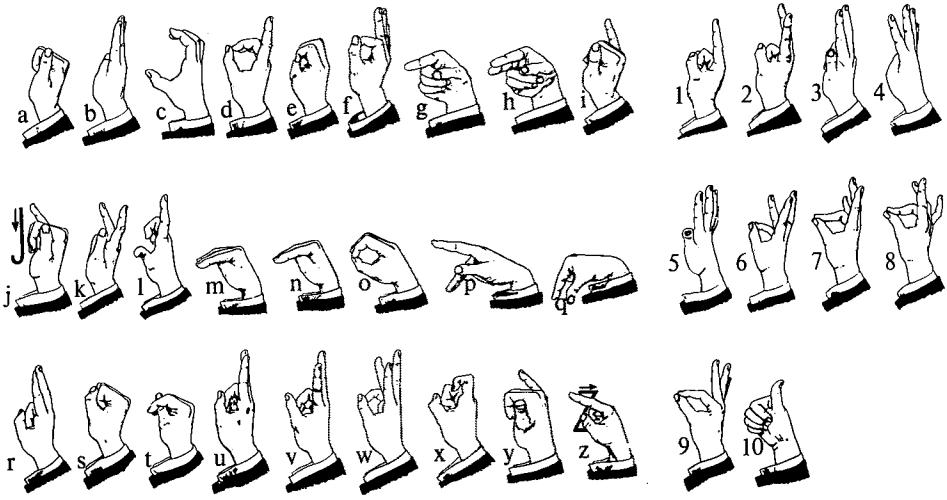
* The research reported in this paper was sponsored by a grant from the Canada Council. I greatfully acknowledge the contribution of Margaret Bick, who did the transcription work and assisted in the data analysis.

Figure 1

VISIBLE ENGLISH SYMBOLS As seen by the sender



VISIBLE ENGLISH SYMBOLS As seen by the receiver



in reading one perceives the word as a whole. This results in a type of language acquisition that is unique. People learning to fingerspell can produce the language before they comprehend it.

In spoken language phonological variation can be due to either or both of two factors — it can be due to some universal principles of what is physically possible (or at least easier) given the anatomical and neurological facts of the speech related organs, or it can be due to arbitrary historical fact, passed on from one generation to the next. The latter is possible because the child learns to perceive the language before he can produce it, and thus can learn not only the sounds in isolation, but also the transitions and variations. The same two factors can be present in a deaf child learning fingerspelling as his first language. However, a hearing adult learning fingerspelling has only the letter configurations to go on. If there is consistency across adults learning to fingerspell it is more likely due to neurophysiological facts than it is to language history. Thus a study of the similarities and differences between phonology and fingerspelling can help us separate out general psychological principles from historical accident.

Another advantage of studying fingerspelling is that the articulatory mechanism is visible. Thus we can determine what is going on without complicated equipment or discomfort to the informant.

Method

Samples of Visible English were collected from 14 teachers at two schools for the deaf in the U.S. On my request, I was taken to those teachers who the principals felt were their best teachers. The classes observed ranged from preschool through high school. Each class was videotaped with a SONY Portapack camera and taperecorder. The teachers were asked to conduct their classes as they normally do, and this in general they did, though in some classes I was prevailed upon to answer questions about Eskimos, hockey and other things Canadian.

Back in the laboratory, between 100 and 125 words of continuous text were transcribed for each teacher. Both the spoken English and the hand configurations were transcribed. The hand configurations were transcribed by putting the taperecorder on pause and manually moving the tape frame by frame. The notation used was developed for the purpose. It consisted of the alphabet, each symbol of which corresponded to the hand configuration shown in the figure, plus subscripts which described how the hand varied from the "normal" form.

Evidence Favoring a Phonological Analysis

Under what conditions is it appropriate to do a feature analysis of a language? Features are useful if one finds: first, that all phonemes with a particular feature behave the same way in particular environments; second, all phonemes with a particular feature serve as a conditioning environment for some phonological alternation; and third, the feature has some discernable relationship to some articulatory or auditory fact. In other words, features are useful if they simplify the description of the grammar and have some face validity.

In particular, certain phenomena are consistently discussed as epitomizing what can happen in phonology — for example, the choice of a particular

allophone may depend upon its position — initial, medial or final — in a word or syllable; it may depend upon the nature of the surrounding phomes, it may depend upon whether the syllable is stressed or unstressed; there can be either forward assimilation or backward assimilation or both; and there may be metathesis, in which sounds get reordered.

Do comparable things happen in fingerspelling? Yes, they do. Consider the following examples: (a) allophonic variation with position: We have not yet discovered any consistent differences in the shape of letters, based on whether the letter is initial, medial, or final, despite the fact that people who fingerspell believe that final letters are more "accurate". However, there are consistent regular differences in the length the letter is held. The shortest length is the length of the letter in medial position. The next length is used for word initial and word final position. There do not appear to be resular differences in length between these two categories. Sentence initial letters appear to be held longer than word initial or word final. By far the longest held letters are those that are sentence final.

Table 1 shows the figures for all occurrences of letters *t*, *e*, *s* and *h* in the segment of one informant's speech which we have analyzed. The speech of other informants appears to show the same four lengths. These figures count "missing" letters — those letters that exist in the orthographic spelling but not in the fingerspelling, except perhaps in the modification of surrounding letters — as having a duration 0, but the conclusions are not altered if these examples are excluded. Letters that were sentence initial were not also included in the count for word initial, and similarly for word and sentence final. Double letters have been excluded. The figures are given in number of "frames" in which the letter was visible. On our TV equipment there are 60 frames per second.

TABLE 1
Length in Frames Letters are Held

	Letter (number of occurrences in parentheses)				
	t	e	s	h	mean
Sentence Initial	4.67	-	4.67	7.33	5.55
	(3)		(3)	(3)	(9)
Word Initial	3.25	-	3.10	5.33	3.71
	(8)		(10)	(6)	(24)
Word Medial	1.62	1.07	0.33	2.00	1.34
	(13)	(27)	(3)	(10)	(53)
Word Final	4.67	1.84	8.00	4.00	3.93
	(9)	(13)	(5)	(1)	(28)
Sentence Final	23.33	13.00	22.50	60.00	25.12
	(3)	(2)	(2)	(1)	(8)

The same pattern is true if the time for the transition between letters is taken into account. Table 2 shows the length of a letter for the same informant as defined by the time a letter is held plus half the transition time between it and the preceeding and following letters. Sentence initial and final letters are not included because it is difficult to determine the transition between, for example, the last letter of a sentence and extralinguistic activities such as scratching one's nose or picking up a piece of

chalk to write on the blackboard. In this table letters that did not occur are excluded.

TABLE 2
Length of Letters Including Transition Time

	t	e	s	h
Word Initial	9.62	-	10.41	12.41
	(8)		(6)	(6)
Word Medial	7.50	7.78	4.50	6.62
	(9)	(14)	(1)	(8)
Word Final	10.17	8.50	14.00	9.00
	(9)	(7)	(5)	(1)

Thus we see that position in the word causes consistent variations, just as is true in spoken language.

Are there allophonic variants due to particular surrounding "phonemes"? Yes, there are. Here a feature dimension which we call "fingeredness" appears to be useful. Fingeredness refers to the number of fingers relevant to a particular letter. For example, /h/ is a two-fingered letter, because the position of the index and the middle fingers seem most important. The positions of the ring finger and pinkie can vary considerable without affecting the identification of the letter. Other two-fingered letters include /k/, /p/, /r/, /t/, /u/, /v/ and /n/. On the fingeredness dimension it is useful to identify five types of fingeredness: 1-fingered, 2-fingered, 3-fingered, 4-fingered and pinky-handed. 4-fingered letters such as /e/ and /o/ can be affected by surrounding 2-fingered letters, just as the /æ/ in /mæn/ is affected by the surrounding nasals. For example, in a word like /never/, both /e/'s are surrounded by 2-fingered letters. In such an environment both /e/'s tend to become 2-fingered.

Similarly, 4-fingered letters tend to affect 3-fingered letters. For example, in /remember/, the /m/'s are surrounded by /e/'s and /b/, both 4-fingered. In this environment we have observed 4-fingered /m/'s.

Is there something corresponding to stress effects such as vowel reduction? We have found no consistent effect of letter reduction or dropping due to stressed versus unstressed syllables in the simultaneously produced spoken language; however, we have noticed a similar phenomenon in our informants' word dropping behaviour. Under pressure of time, caused by the relative slowness of the hand compared with the oral articulators, whole words tend to be dropped. There are significant differences in amount dropped depending on syntactic category. When we distinguished "content words" as *nouns*, *adjectives*, *verbs* (excluding auxiliaries), and *adverbs*, from "function words", comprising all other words, we found that function words are dropped significantly more often than content words.

There is evidence of both forward and backward assimilation. The 2-fingered and 4-fingered features can move in both directions, though the effect occurs most often when a letter is surrounded by 2- or 4-fingered letters. The twist of the /g/ and /h/ tends to affect both preceding and following letters, especially on the /wh-/ question words and /the/. Twist of /g/ seems to affect letters following more often than letters preceding.

Other features also move in one direction or another. Most often these are ideosyncratic to particular informants. For example, forward assimilation occurs in thumb-tuckedness in two informants, while in one informant each the features of wrist-bentness, finger-outness, and thumb-&-pinkie-togetherness forward assimilate. In the other direction, thumb-outness in two informants, and finger-apartness and thumb-pinky-togetherness in one informant each appear to undergo backward assimilation.

We do not have any evidence of regularities in the metathesis we found. Metathesis appears as one feature in a general degradation of the message, which also includes adding and dropping letters and hitting the wrong letters. Some examples are shown below:

/nemd/	*named*
/snod/	*sound*
/wintg/	*writing*
/lnrd/	*learned*
/franicton's/	*fraction's*
/ofur/	*four*
/hydrophoiba/	*hydrophobia*
/poele/	*people*
/psre/	*Pasteur*

Implications for Phonology

Thus we see that we find the same phenomena in fingerspelling that we find in the phonology of vocal speech. This fact combined with the visibility of the articulators suggests that an analysis of fingerspelling might lead to insights into the phonology of spoken language. And indeed this appears to be the case.

One feature of phonology which appears to be consistent across all major theories is that the phoneme or some other comparable unit is said to consist of a set of simultaneous features. Thus /n/ is said by Chomsky & Halle to consist of plus consonantal, anterior, voice and nasal, and minus all other features. The matrices that represent sequences of utterances are lined up in columns, each column representing one phoneme. Presumably all features start simultaneously at the beginning of the phoneme and terminate simultaneously at the end of the phoneme. If a feature continues over a larger region, such as an entire consonant cluster, a rule is postulated which copies that feature onto the other positions.

One thing that observation of fingerspelling makes crystal clear is that different features of a letter in general neither start nor end simultaneously. In fact, there are very consistent patterns of what features start in what order.

Consider just two examples from the transcription of a single informant. The letter /l/ consists of the index finger up and the thumb out. In this informant both finger and thumb start to move simultaneously in word initial /l/ and in the word /al/ (*all*). In the case of all other occurrences, the thumb moves out before the index moves up. Thus we find *well* fingerspelled /we$_L$l/, where the thumb during the production of the /e$_L$/ is out rather than tucked.

A second example appears in a comparison of the informant's /u/'s and /r/'s. This informant does not cross his index and middle when forming his /r/'s, so that in stop motion the two letters look alike. However when he produces a /u/ both index and middle move up simultaneously, while when he produces an /r/ the middle precedes the index. Here we have a minimal pair based not on what features are present but on the order in which the features appear.

In general, I am finding that the analysis that is most efficient differs from *Sound Pattern* phonology in five ways. First, features may be sequential as well as simultaneous. Second, it is useful to group features such that only one of the group can occur at any one time. For example, the thumb cannot be both *out* and *tucked*. It is useful to describe five mutually exclusive positions for the thumb. One can represent this notationally either as a single 5-valued feature, or as five unary features connected in a relational network to the same downward disjunction node. Third, it is useful to categorize features of a particular letter into two types — *distinguishing* and *normal*. Those features are distinguishing which are necessary to keep the letter present in the message. Those features are *normal* which position the rest of the hand out of context. For example, for our informant who produces /we$_L$l/, finger tuckedness is *distinguishing* for /e/ but thumb tuckedness is merely *normal*. Fourth, it is useful to assume that the features are *instantaneous onset* features. By this I mean that they signal the onset of a particular feature but say nothing about its duration. In the system I am proposing, an articulator stays in a particular position until a *distinguishing* feature in the same category is encountered. Fifth and finally, it is useful to assume the existence of a feedback feature which signals that a phoneme has been realized and the next phoneme may begin.

There is not time today to give a complete justification for this system. Rather I will confine myself to the question of what insights such a system might provide for a phonology of spoken language — in particular, English.

Application to English Phonology

Does there exist in English phonology a minimal pair in which the only difference is the order of onset of the features? The answer is yes. Consider the following two words: /kidn/ as in "I was only kidding", and /kin/ as in "kith and kin". Looked at in terms of simultaneous onset features, an /n/ is made up of at least three features, *nasal*, a feature that signals the opening of the passage from the mouth to the nose, *coronal* or some other feature referring to the location the tongue moves to, and *stop* (or minus continuant if you prefer). In this system an /n/ consists of *nasal* followed by *coronal stop*. If the sequence is reversed, we get the final consonant cluster of /kidn/.

What advantage is there of representing /n/ in this way? Consider Chomsky & Halle's (1968:165) discussion of the word *inn*. On the feature *nasal*, they say that the /i/ has a value "2", indicating that "the degree of nasalization is partial". This is the representation they give in the "phonetic representation". /i/ is presumably minus or 0 nasal in the "phonological representation". Rules must be postulated to convert from the "phonological" to the "phonetic" representation.

What does "partially nasalized" mean? What it means, in fact, is that when the /i/ begins, it is not nasalized, but when it ends it is nasalized.

From the perspective of instantaneous onset features all this is a cumbersome and *ad hoc* way of expressing the fact that in English all nasal consonants are produced in two steps — first the nasal passage is opened and second the oral passage is closed. Here is a case where onset features considerably simplify the phonology.

Consider the nasalization of vowels between nasal consonants in such words as *man*, *men*, *nim*, and *none*. In the instantaneous onset feature framework no rule must be postulated to place the nasality feature on the vowel. Vowels are *normally* minus nasal, but nasality is not a *distinguishing* feature, so the nasal onset of the previous consonant simply remains until some other consonant appears that has minus nasal as a distinguishing feature.

Consider another example. It is well known that the lip position during the /g/ of *goose* is different than the lip position of the /g/ of *geese*. Again in an instantaneous onset feature phonology no feature movement rules are necessary. Lip position is not distinguishing, and if it is assumed that lip position is a feature which precedes other features of a vowel just as nasal precedes other features of a nasal consonant, the lip position of the vowel would be superimposed over the initial consonant.

It is also interesting to look at the behaviour of /l/ in consonant clusters in terms of the instantaneous onset features. In such a system /l/ has two allophones. Initial /l/ has as distinguishing features *coronal stop* and *lateral*. Final /l/ has as distinguishing feature *uvula constricted*, and it has the initial /l/'s distinguishing features as *normal* features. Thus the tongue touches the roof of the mouth if the final /l/ stands isolated from other consonants, such as in /wélawér/ (*well aware*), but not when contiguous with other consonants as in /help/ or /welbred/ (*well bred*). On the other hand, initial /l/'s distinguishing features overshadow the normal features of the initial consonants /p/ and /b/. Thus in /pliyd/ and /bliyd/ the tongue is in the /l/ position before the release of the previous consonant. The feedback feature is needed to describe the fact that the tongue moves into the /l/ position *before* the release of the consonant but after the *bilabial close*.

Theoretical Implications and Conclusion

Just a word or two about the relationship between instantaneous onset features and stratificational grammar. Although an instantaneous onset feature phonology can be developed within any linguistic theory, I see it as solving several theoretical problems for a stratificationalist. Lamb is fond of pointing out the similarities of one stratum to another, and yet phonology always looked different because the features were simultaneous rather than sequential and had a discrete length rather than being instantaneous signals. A stratificational grammar with an instantaneous onset feature phonology is more consistent and parsimoneous than one without.

In this paper I have tried to demonstrate that fingerspelling undergoes the same phenomena to which phonological systems are subjected. Thus such processes are shown to be psychologically based rather than merely historically artifactual,

and such processes are also shown to be modality independent. This fresh look at a radically different phonological system has caused a re-evaluation of some of the basic assumptions of how a phonology is constructed. This new approach has led to a simplification in the way the assimilation of features to neighboring phonemes is described. This new approach also results in an ultimate featural description which is closer to the articulatory reality than any other approach. It is for these reasons that I find an instantaneous onset feature phonology not only a viable but a preferable alternative to standard phonologies.

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Verbal Responses to Body Language

J. H. Prost and S. L. Smith
University of Illinois, Chicago Circle

In an earlier project a subject was asked to take poses in response to commands from the investigator. The commands were emotional words, such as, happy, sad, angry, afraid, surprised, relaxed, tired, disgusted, arrogant, etc. In some of the sessions the subject was asked to stand while posing, in others, to sit on a chair or sit on the floor. The subject's poses were photographically documented. The objective of the study was to analyze the biomechanical attributes of expressive postures (Prost, 1974). The fact that the subject was only acting the emotions was of no concern because for a study of balance, limb mechanics, or range of joint motions, subjects have to meet the same physical laws whether they really feel the emotions or not, gravity being invariant to emotional states.

In the course of the work, the investigator felt that some of the poses were quite realistic, so much so that anyone looking at the photographs might easily identify the intended emotions. The slides were shown to a number of respondents to test this impression. There were a few poses for which most of the respondents guessed the emotional words given the subject, e.g., happy seen as 'happy,' etc. There were a few others for which almost all used the same words but not the commands given the subject, e.g., surprised seen as 'happy,' etc. There were many for which there was little agreement and almost no identification of the command words, e.g., happy seen as 'pleased,' 'sad,' 'nervous,' 'shocked,' etc.

This large set, having low interrespondent agreement and low identification of the intended emotions, is the bane of research on body language. Some workers ignore it while others design methods to avoid it (Darwin, 1872; Allport and Vernon, 1933; Ekman, Friesen and Ellsworth, 1972; and Ekman, 1973). One common method is to assume that it arises from synonymic usage; in which case, it is reduced, or eliminated, by collapsing synonyms into higher level categories of 'meaning.' 'Free responses in the observer's own words have been less popular in recent years, perhaps because of Woodworth's finding that organizing such free responses into categories yielded considerably higher agreement ...' (Ekman, Friesen and Ellsworth, 1972, p. 45).

Turning in a different direction, we were curious about those poses for which the respondents achieved high agreement, where free responses gave high interrespondent agreement, with or without agreement with the command words. We were not curious about the poses themselves. We were curious about the words the respondents used. On what words did the respondents agree most? On what words did the respondents agree with the commands?

These two questions seemed important because from the earlier work we had data to show that, even with free responses, observers reach very high agreement for a small set of words and for this set only. If this set were to have no synonyms there would be no justification for the strategy of 'organizing free responses into categories.' We suspected that this set held a special status in people's vocabularies. It designated the cultural

standards for the people acting as respondents. When respondents used words other than those from this set it implied that the respondents saw a distinction between the poses being observed and their expectations with respect to the standards. If this interpretation could be validated then the strategy of collapsing 'synonyms' would have no justification.

To explore this possibility, a new sample was taken. Several subjects were asked to pose. The respondents were picked to get people from close and distant speech communities, to see if agreements in close speech communities involved the small set rather than the vocabularies as a whole.

Research Design

Three subjects acted as the posers. One was a young girl, Kimmy, photographed when she was 12 years old (Plate I,1) and again when she was 13 years old (Plate I, 2-8). One was a young boy, Jack, Kimmy's brother, photographed when he was 12 years old and his sister was 13 (Plate II, 1-8). The last, Steph, was a woman of 32 years.

The subjects were asked to act out emotional words, such as, happy, sad, angry, surprised, afraid, etc. They were asked to do this while standing, posing on a chair, or posing on the floor. The emotional words and postural states were randomized and matched so that all the emotional words were given in conjunction with all three postural states. The chair was the same for all the poses, a straight-backed, wicker-seat, dowl chair.

The subjects were asked to take poses which would, in their opinions, communicate the emotional word to an audience. As they assumed their poses they were photographed. The subjects were given the same commands over a period of time, on alternate week-ends, so their performances would not be constructed all on the same day. The samples are identified in the rest of the paper with the letters 'Kb' for Kimmy's earlier performances (275 poses), 'Kc' for Kimmy's later performances (535 poses), 'Sa' for Steph's performances (556 poses), and 'Jb' for Jack's performances (480 poses).

The photographs were shown to a number of students who were asked to interpret in one word, if possible, what the subjects were communicating. The respondents were told that, although the subjects were trying to communicate, they were not professional actors and not always accurate; therefore, the respondents should not worry about right or wrong answers. The respondents were given the instructions: 'What we want to know is what these untrained subjects really ended-up saying with their faces and bodies. For this, you are the authorities and whatever you think you see is the absolutely correct answer. Just say whatever comes first into your mind, no matter how silly you might think it sounds.' The instructions were meant to relax the respondents and put them in a state of free response.

The respondents were members of the student body of the University of Illinois, Chicago. The whole sample was divided into groups of 80 slides. Ten students, 5 males and 5 females, viewed each group of 80 slides. It was found that students could not sustain viewing attention for longer than 40 to 50 minutes, and only 80 slides could be shown in that time. No student was allowed to see the same subject more than once, so no student saw more than 80 slides of the same subject. The student responses for each sample came from a minimum of 60 students per subject. The students' responses will be called the public.

Kimmy and Jack were asked for their responses on the entire sample. They, of course, had to be told to try to guess the command words since they presumed

they were in a contest for accuracy anyway. Because of their young ages their concentration time was less and they were shown the sample, in groups of 60 slides, with 30 minute sessions. They went over the sample two months after they had finished their posing. They were never told they would view their own performances, hopefully, to cut down intentional memorization of their habits. Kimmy was shown her slides of one year earlier, when she was 12 years old. Jack also saw the Kb sample and was, therefore, viewing his sister's performances of one year before.

The public saw the sample without any knowldege of the command words and they never had any contact with any of the subjects. Kimmy, Jack, and Steph had grown up in Los Angeles, California. The public was mostly from Chicago, Illinois. Kimmy and Jack, having been the subjects, were familiar with the command words. Both Jack and Kimmy viewed the entire sample, their own slides as well as those for Steph, while the public saw only a fraction of any one subject's poses. It was thought desirable to have at least one student view the entire sample. The subject was an adult woman, Sandy, 21 years old, a resident of Chicago, who had grown up in Chicago and was a student at the University. She had no prior knowledge of the experiment and knew none of the subjects.

There were four samples of photographs: Kb, Kc, Sa, and Jb. There were four respondent categories: Sandy, Kimmy, Jack, and the public.

The responses of all the respondents were dealt with in the same manner. If the respondent described the pose with one word, that word was recorded as the response. If gramatically different forms of the same stem occurred, they were changed into the more frequent usage, e.g., joy, joyful, and joyous became joyful. Synonyms were not made equivalents, e.g., happy and joyful were retained as two different responses.

If the respondents used two words, the words were reduced to a single word. If the respondents used the command term as one of their words, the command was scored as the response. If neither word was identical to the command, then the one chosen was the one which agreed with the greater number of other responses on the same pose. If neither of the two words agreed with the command nor any of the other responses on the same slide, then the word scored was the one which had been used with highest frequency for the sample as a whole. All multiple word responses were dealt with in this manner.

If a phrase, or sentence, was used the structure was searched to discover a form of the command, lacking this, a form of one of the words used by the other respondents. If this failed then whatever word was contained in the sentence, most clearly representing what the investigators thought was the intention of the phrase, or sentence, was scored as the response. In a few rare cases the phrase or sentence could not be reduced by any of these procedures, e.g., Just got a million dollars; in which case, the investigators tried to guess the meaning of the phrase, or sentence, and assign a single word expressing that meaning, using first a command word if that prooved appropriate, or a frequently used word for the sample as a whole. For example, Just got a million dollars reduced to happy for a pose where the command was happy or, reduced to happy where one or more other respondents said happy on the same pose or, reduced to joyful if no respondent said happy but some said joyful or, reduced to happy if no positive word had been used for the pose because happy was used frequently by the public throughout the sample.

The objectives behind these reductions were 1) to get a single word for each response, 2) to maximize agreement between the command terms and the responses or, lacking that, 3) maximize agreement between respondents. Only

20% of the responses required reduction and in less than 3% was it necessary for the investigators to exercise any kind of judgment.

Analysis

Tables 1, 2, 3, and 4 show the results of the analysis. On the left of each table, in the first column, is a list of the words used by the respondents: Kimmy, Jack, Sandy, and the public. The other columns show the word usage percentages listed under headings describing the conditions of their usage. Words used less than 1% of the time are not included.

The index row (the first row) gives the various conditions of agreement. The second row contains the total number of words used under each of the conditions specified in the index row. The second row gives the percentages for these totals. In the rest of the table, the percentages in each column refer to the contribution of each individual word to the Total Number given in the second row.

The first table tabulates the responses and agreements on the Kb sample. This sample had a total of 275 slides. The number of commands given the subject was 275 (indicated under the column heading 'Command'). Reading down the column, the word happy was used as a command 7% out of the 275 commands given, while relaxed was used only 6% out of 275. Toward the end of the column are words which were not used as commands but were used frequently by the respondents. Disappointed was not used as a stimulus command but Sandy used this word in 10% of her responses. Further down the column is the word confident which was neither a command nor was used by Sandy. Kimmy used confident 5% of the time. Kimmy's brother, Jack, and the public, rarely used confident, as indicated by the dashes along the row. Kimmy was the only person to use confident to any appreciable degree.

In the third column we have the total number of times Sandy agreed with the commands (i.e., 'Sandy Agrees with Command'). Sandy guessed the command words 77 out of 275 times making a percentage agreement of 28%. Kimmy, the next column, agreed 90 out of 275, or 33%. It is reasonable that Kimmy should have done better than Sandy since Kimmy was viewing her own postures, one year later, however. Jack agreed with the commands 25% and the public agreed 18%. It is interesting that Jack read his sister better than the public but no better than Sandy. One would suspect a brother could read his sister better than someone drawn at random from the public.

We can compare the contributions of each word to Sandy's performance, under 'Sandy Agrees with Command.' Happy contributed 17% of Sandy's agreement with the command words, that is, Sandy guessed the command happy 17% of her 77 correct guesses. Kimmy guessed happy 14% of her 90 correct guesses. Jack guessed happy 17%. The public got happy 23%.

The public had trouble with afraid, but Sandy, Kimmy and Jack guessed afraid very well. Jack guessed sad far better than did Kimmy, who was viewing herself. Jack also did well with angry. Kimmy, after a year, totally misread her 'shy' performances, although Sandy, Jack and the public did quite well.

Overall, the correct guessing of the commands by Sandy, Kimmy, Jack, and the public revolved around a few words like happy, afraid, sad, etc. The differences in the abilities of the respondents to guess the commands reflected the amount of correct guessing for a few words rather than an accumulation over the total variety of words used.

Table 1. Contributions of Each Expressive Word to the Agreements Between Respondents

Kb Sample: Agreement Categories

	Command	Sandy Agrees with Command	Kimmy Agrees with Command	Jack Agrees with Command	Public Agrees with Command	Sandy's Words	Kimmy Agrees with Sandy	Jack Agrees with Sandy	Public Agrees with Sandy	Kimmy's Words	Jack Agrees with Kimmy	Public Agrees with Kimmy	Jack's Words	Public Agrees with Jack
Total Number:	275	77	90	69	500	275	61	57	468	275	96	472	275	444
Percentage:		28%	33%	25%	18%		22%	21%	17%		35%	17%		16%
						Percentage Contribution of Each Word to Total								
Word List:														
happy	7%	17%	14%	17%	23%	9%	30%	37%	33%	9%	23%	33%	19%	41%
afraid	7	12	19	20	6	4	15	12	3	8	18	6	7	8
relaxed	6	1	11	-	4	3	2	-	2	13	-	5	<1	<1
sad	6	1	9	20	7	1	-	4	1	12	25	7	28	16
sick	6	9	3	3	2	3	3	2	<1	1	1	-	<1	-
surprised	6	16	14	10	19	7	16	11	21	6	8	19	4	14
tired	6	8	2	3	5	7	7	4	7	5	4	2	7	2
angry	5	6	7	13	7	2	3	5	3	3	6	4	4	6
thinking	3	4	3	1	3	5	3	<1	5	7	3	5	1	<1
attractive	3	3	1	-	-	4	2	-	-	1	-	-	-	-
unattractive	3	-	-	-	-	-	-	-	-	-	-	-	-	-
feel good	3	-	-	-	3	-	-	-	-	-	-	-	-	-
pleased	3	1	-	-	<1	3	-	-	1	-	-	-	-	-
disgusted	3	1	1	-	-	<1	-	-	-	<1	-	-	-	-
arrogant	3	4	-	-	2	1	-	-	2	-	-	-	-	-
shy	3	9	-	4	3	4	-	5	3	-	-	-	2	2
moody	3	-	-	-	<1	-	-	-	-	-	-	-	-	-
waiting	3	-	-	-	<1	-	-	-	-	-	-	-	<1	-
bored	3	-	-	-	5	-	-	-	-	-	-	-	-	-
puzzled	3	3	3	3	3	<1	3	2	<1	4	1	2	1	-
emotionless	3	-	4	-	4	-	-	-	-	2	-	1	-	-
embarassed	3	1	-	-	<1	3	-	-	-	-	-	-	-	-
unhappy	3	4	-	4	<1	5	-	4	<1	-	-	-	3	-
silly	3	-	-	-	-	-	-	-	-	-	-	-	-	-
tense	2	-	1	-	1	<1	-	-	<1	<1	-	<1	-	-
guilty	2	-	4	-	-	-	-	-	-	7	-	<1	<1	-
sexy	<1	-	-	-	-	<1	1	2	-	1	2	-	3	<1
good girl	<1	-	-	-	-	-	-	-	-	-	-	-	-	-
singing	<1	-	-	-	1	-	-	-	-	-	-	-	-	-
disappointed						10	-	-	4	-	-	-	-	-
mad						8	2	2	<1	1	1	<1	3	<1
thoughtful						5	3	2	5	<1	3	5	1	<1
pain						4	8	7	6	4	5	9	2	7
shocked						2	-	-	1	-	-	-	-	-
relieved						2	-	-	-	-	-	-	-	-
proud						1	1	-	4	3	-	4	-	-
looking						1	-	4	-	-	-	-	2	-
excited						1	-	-	-	-	-	-	-	-
upset						1	-	-	-	-	-	-	-	-
confident										5	-	-	-	-
formal pose										4	2	-	2	-
unemotional										2	-	1	-	-
pretty										1	-	-	-	-
sleeping													1	-

Table 2. Contributions of Each Expressive Word to the Agreements Between Respondents

Kc Sample: Agreement Categories

	Command	Sandy Agrees with Command	Kimmy Agrees with Command	Jack Agrees with Command	Public Agrees with Command	Sandy's Words	Kimmy Agrees with Sandy	Jack Agrees with Sandy	Public Agrees with Sandy	Kimmy's Words	Jack Agrees with Kimmy	Public Agrees with Kimmy	Jack's Words	Public Agrees with Jack
Total Number:	535	166	233	156	750	535	157	147	764	535	201	832	535	720
Percentage:		31%	44%	29%	14%		29%	28%	14%		38%	16%		13%
						Percentage Contribution of Each Word to Total								
Word List:														
surprised	5%	14%	10%	12%	17%	5%	14%	13%	18%	5%	10%	16%	4%	14%
sexy	5	11	8	15	3	5	9	16	4	3	8	3	9	6
happy	5	12	9	14	21	8	11	13	18	7	15	27	11	36
sick	5	4	-	3	3	3	-	2	1	-	-	-	3	2
guilty	4	-	5	-	-	-	-	-	-	3	-	-	<1	-
hate	4	-	-	-	-	-	-	-	-	-	-	-	-	-
attractive	4	4	4	<1	<1	9	3	<1	<1	4	-	-	<1	-
pleased	4	1	-	2	<1	4	-	-	<1	-	-	-	<1	<1
bored	4	5	<1	-	6	3	2	-	4	<1	-	<1	<1	<1
afraid	4	7	9	13	3	3	8	7	2	5	10	2	5	2
shy	4	7	6	1	4	5	8	<1	5	5	<1	5	1	<1
thinking	4	5	7	8	5	4	10	10	5	8	12	8	9	7
aggressive	4	-	<1	-	<1	-	-	-	-	<1	-	-	-	-
pain	4	8	9	8	11	4	13	8	13	7	10	17	4	13
angry	4	3	-	1	6	4	-	3	7	<1	<1	<1	2	2
puzzled	4	-	6	3	<1	-	-	-	-	5	<1	<1	2	<1
sad	4	-	5	9	3	-	-	-	-	7	8	3	12	2
tense	4	4	6	<1	4	3	2	1	2	4	-	3	<1	<1
tired	4	6	3	9	4	6	3	7	3	3	6	3	8	7
embarassed	4	5	3	-	3	4	3	-	3	2	-	2	-	-
relieved	4	<1	<1	-	1	<1	-	-	<1	<1	-	<1	-	-
relaxed	4	3	3	<1	3	4	5	2	5	6	2	4	3	2
confident	3	-	2	-	<1	-	-	-	-	3	-	<1	-	-
emotionless	3	-	4	-	<1	-	-	-	-	3	-	<1	-	-
formal pose	3	-	2	-	<1	-	-	-	-	3	-	<1	4	-
mad						6	10	14	2	9	15	3	13	2
unhappy						4	-	-	1	-	-	-	<1	-
disappointed						4	-	-	2	-	-	-	-	-
nothing						3	-	-	2	-	-	-	-	-
frightened						1	-	-	<1	-	-	-	-	-
looking						1	-	-	<1	-	-	-	<1	<1
watching						1	-	-	-	-	-	-	-	-
proud						1	-	-	1	1	-	<1	-	-
excited						1	-	-	-	-	-	-	-	-
unemotional										2	-	<1	-	-
sleeping										2	-	<1	1	2
nervous													1	<1

Table 3. Contributions of Each Expressive Word to the Agreements Between Respondents

Jb Sample: Agreement Categories

	Command	Sandy Agrees with Command	Kimmy Agrees with Command	Jack Agrees with Command	Public Agrees with Command	Sandy's Words	Kimmy Agrees with Sandy	Jack Agrees with Sandy	Public Agrees with Sandy	Kimmy's Words	Jack Agrees with Kimmy	Public Agrees with Kimmy	Jack's Words	Public Agrees with Jack
Total Number:	480	120	129	149	555	480	153	115	626	480	166	657	480	461
Percentage:		25%	27%	31%	16%		32%	24%	13%		35%	14%		10%

Percentage Contribution of Each Word to Total

Word List:														
afraid	5%	4%	19%	13%	4%	1%	4%	3%	1%	12%	14%	5%	5%	5%
surprised	5	12	8	5	15	5	4	3	13	3	5	6	3	6
sad	4	-	13	11	11	-	-	-	-	7	11	13	9	14
happy	4	14	14	6	17	11	20	17	31	10	12	29	6	21
attractive	4	4	-	<1	-	3	-	-	-	-	-	-	1	-
relieved	4	-	-	3	<1	<1	-	-	-	-	-	-	3	<1
pleased	4	2	-	3	<1	2	-	-	<1	-	-	-	2	<1
tense	4	4	-	5	1	2	-	3	<1	-	-	-	2	<1
confident	4	-	-	-	<1	-	-	-	-	<1	-	<1	-	-
sick	4	7	6	3	3	4	5	2	4	3	2	2	2	2
hate	4	-	-	-	-	-	-	-	-	-	-	-	<1	-
bored	4	7	2	-	3	5	1	-	3	1	-	<1	-	-
guilty	4	-	2	2	<1	-	-	-	-	<1	-	-	<1	-
sexy	4	5	2	7	-	2	2	4	-	1	3	-	4	<1
shy	4	7	7	6	2	6	5	8	4	3	5	4	7	4
thinking	4	11	9	3	5	7	14	5	6	5	4	4	3	5
tired	4	13	2	5	7	13	7	18	14	4	5	3	7	8
emotionless	4	-	-	<1	<1	-	-	-	-	5	-	<1	<1	-
relaxed	4	<1	3	1	1	3	-	<1	2	7	3	4	3	2
aggressive	4	-	-	<1	3	-	-	-	-	-	-	-	<1	<1
pain	4	6	8	6	10	3	8	8	9	4	6	11	6	15
angry	4	<1	-	<1	9	2	<1	2	4	<1	-	<1	2	6
embarassed	4	2	<1	-	2	3	<1	-	1	1	-	<1	-	-
puzzled	4	-	4	7	5	-	-	-	-	2	3	1	5	5
formal pose	4	-	<1	10	-	-	-	-	-	<1	<1	-	7	-
mad						10	30	27	2	15	24	4	9	4
unhappy						8	-	-	1	<1	-	-	<1	-
disappointed						3	-	-	<1	-	-	-	-	-
frightened						2	-	-	1	-	-	-	-	-
nothing						2	-	-	3	-	-	-	-	-
looking						1	-	-	<1	-	-	-	<1	-
proud						<1	-	-	-	2	-	2	-	-
sleeping										6	1	9	<1	2
unemotional										5	-	<1	<1	-
don't know										<1	<1	<1	6	-

Table 4. Contributions of Each Expressive Word to the Agreements Between Respondents

Sa Sample: Agreement Categories

	Command	Sandy Agrees with Command	Kimmy Agrees with Command	Jack Agrees with Command	Public Agrees with Command	Sandy's Words	Kimmy Agrees with Sandy	Jack Agrees with Sandy	Public Agrees with Sandy	Kimmy's Words	Jack Agrees with Kimmy	Public Agrees with Kimmy	Jack's Words	Public Agrees with Jack
Total Number:	556	220	226	171	1051	556	269	174	1092	556	193	1028	556	794
Percentage:		40%	41%	31%	19%		49%	31%	20%		35%	18%		14%
						Percentage Contribution of Each Word to Total								
Word List:														
surprised	5%	13%	12%	8%	14%	6%	11%	7%	15%	6%	8%	16%	4%	10%
happy	5	12	10	8	13	8	14	11	19	8	9	18	5	16
angry	5	-	-	3	15	-	-	-	-	<1	-	-	1	3
afraid	5	12	12	14	5	7	12	17	7	9	17	7	11	7
sad	5	-	5	6	3	-	-	-	-	3	3	1	5	2
sexy	4	10	9	11	8	6	7	14	10	4	9	8	10	22
hate	4	-	-	-	<1	-	-	-	-	-	-	-	<1	-
shy	4	3	6	4	3	2	3	2	2	5	4	4	3	3
relieved	4	-	-	2	<1	-	-	-	-	-	-	-	2	<1
tense	4	5	<1	2	<1	4	<1	2	<1	<1	-	-	2	-
guilty	4	-	-	-	-	-	-	-	-	<1	-	-	<1	-
attractive	4	5	6	2	-	4	3	3	-	4	2	-	3	-
pleased	4	2	-	5	<1	<1	-	<1	-	-	-	-	2	<1
sick	4	5	<1	4	2	3	<1	1	2	1	<1	<1	4	2
thinking	4	3	5	6	3	6	8	11	6	8	11	7	11	10
tired	4	9	7	1	4	8	7	1	7	5	1	4	<1	<1
aggressive	4	-	<1	-	<1	-	-	-	-	<1	-	<1	-	-
pain	4	8	8	6	13	5	9	7	14	6	11	18	5	15
embarassed	4	1	<1	-	<1	2	-	-	<1	<1	-	-	<1	-
puzzled	4	-	3	6	4	-	-	-	-	3	1	<1	6	4
confident	4	-	2	<1	<1	-	-	-	-	<1	-	<1	<1	<1
bored	3	4	1	-	4	3	1	-	3	1	-	1	-	-
emotionless	3	4	7	-	2	-	-	-	-	5	-	3	-	-
relaxed	3	5	4	3	3	5	5	2	6	4	2	5	2	1
formal pose	3	-	3	9	-	-	-	-	-	2	4	-	6	-
mad						11	16	19	4	11	18	4	11	4
unhappy						7	-	-	<1	-	-	-	-	-
nothing						4	-	-	3	-	-	-	<1	-
disappointed						3	-	-	<1	-	-	-	-	-
arrogant						2	-	-	<1	-	-	-	-	-
proud						2	3	-	1	3	-	2	-	-
unemotional										5	-	3	-	-
distressed										3	-	<1	-	-
scolding										2	-	<1	-	-
grateful													2	-

If we ignore the commands given and concentrate only on the words used by the respondents we can calculate the amount of agreement between the various respondents. In the column labelled 'Sandy's Words' we have a listing, as percentages, of the words Sandy used to describe the 275 poses. Sandy used many of the command words plus a few of her own. Some of these extra words were also used by Kimmy, Jack, and the public. The percentage agreement between Sandy's usage and the other respondents are given in the three columns following 'Sandy's Words.'

It is seen that Kimmy, Jack, and the public agree heavily with Sandy in their naming of 'happy' poses. Note that Sandy had 9% happy whereas only 7% of the poses were generated by the command happy. There were, then, some poses which Sandy thought were 'happy' but were actually generated by other commands, such as feel good, or pleased. Overall, the public agreed with Sandy no better than it did with the original commands. Kimmy and Jack did no better either, and Kimmy's excellent guessing drops.

Why does the public agree so poorly with Sandy, someone from their own language community? Why do Jack and Kimmy agree so well with Sandy? These results come from high agreements between Sandy, Kimmy, and Jack on a few words. Sandy's vocabulary is more similar to the publics', in terms of the various words used, but Sandy, Kimmy, and Jack agree on more poses for a few key terms. These are clearly cases where evaluation of the postures, and not synonymy, control the agreements.

If we turn to Kimmy's words we find a major use of the command words, and a few of her own, like confident, or unemotional. Jack agrees with Kimmy very well, better than any other agreements throughout the table, a consequence, undoubtedly, of the fact that they are brother and sister. But this high agreement does not reflect a similar vocabulary as much as it reflects unanimity in the interpretations of the poses.

Jack's vocabulary is small, probably due to his young age. His percentages rely heavily on his usage of a few words. Sandy and Kimmy's agreements with Jack are found earlier in the chart, since Jack's agreements with Sandy are the same as Sandy's agreements with Jack.

Table 2 is for the Kc sample, Kimmy performing one year after the Kb performances, and 2 months before she responded to the slides. As expected, her ability to guess the commands improved dramatically; she had just finished hearing the commands. She was able to guess more of the commands, although she never guessed any one command any better than she did for her earlier performances. Sandy, Jack, and Kimmy all guessed better on this sample. The public did worse. Kimmy was older and her posing categories were better structured. Jack was better able to read his sister across a larger variety of pose categories, although he was unable to guess 'happy' and 'sad' as well as he did for her earlier performances.

Table 3 shows the same data for Jack's performances (Jb). Jack did best with his own behaviors. Kimmy did next best, with her brother. The public had trouble with Jack's vocabulary, the lowest percentage of all.

The last table is for Steph, who prooves to be the best performer, also the only adult performer. Everyone guesses better and agrees well with each other, except for the public's agreement with Jack's vocabulary. The star performance is between Kimmy and Sandy who agree 49% of the time while Sandy's peers can only agree with her 20% of the time. These percentages are unexplainable except to note that most of this agreement lies with Sandy and Kimmy's use of the word mad. Many of Steph's angry and hate poses were called mad by Kimmy and Sandy and their transference from the group angry to the

group mad accounts for a significant percentage of the agreement between Kimmy and Sandy. The public used the word angry and did not follow Sandy in translating angry into mad. This is a clear case of disagreement through synonymy. Jack liked mad, even more than Kimmy, but, like the public, agreed less overall with Sandy's assessments.

In order to understand the structure behind the agreements, the percentage contributions of each word were plotted against the number of words which accounted for the total amount of the agreements (Figure 1). The agreement categories were plotted separately for each sample. The curves are very similar. The ordinates show accumulative percentages; the contributions of each word are added sequentially. The abscissa charts the number of words entering the accumulative percentage. The words in Tables 1, 2, 3, and 4 were ordered according to their amounts of contribution, higher contributions coming first, and then plotted.

The Figure shows clearly that the agreements, of whatever category, are the result of the contributions from a few words. Ten or less words account for 80% to 90% of the agreement. 20, or less, account for all, or almost all, of the agreement. These heavily weighted words are in common throughout the respondents' vocabularies and the final amount of agreement in any category derives from the application of these common words to postural groupings.

The curves suggest that the ability of an observer to read another's expressive behaviors relies on the talents of the performers to create identifiable groups of equivalent poses. The observers have a vocabulary containing truncated sets of frequently used common words. If a performer can create neat groupings, interrespondent agreement increases because the neat groupings increase common applications of the truncated set. Synonymy plays but a secondary role.

A selection from the poses are given in Plates I and II. All the poses are ones for which public agreement was over 60%. Plate I,1 shows the subject Kimmy posing to the command happy. 70% of the public guessed happy. Sandy, Kimmy and Jack all guessed happy. 20% of the public said laughing and 10% said gleeful. The first pose comes from the Kb sample where Kimmy was 12 years old. The rest of the poses in Plate I are from the Kc sample where Kimmy was 13 years old. Pose 2 is again a response to the command happy. 90% of the public said happy and 10% said delighted. Kimmy, Jack and Sandy all said happy. Pose 3 is for the command surprised and 90% of the public guessed surprised, 10% singing. Kimmy, Jack and Sandy all guessed surprised. Pose 4 is for the command pain and 80% of the public got pain, 10% nauseated and 10% sick. Kimmy and Sandy guessed pain while Jack guessed sick. Pose 5 is the command puzzled. 60% of the public said thinking as did Kimmy and Sandy. Jack guessed puzzled. Some of the public guessed reflecting, daydreaming, quiet and indifferent. Pose 6 is to the command aggressive with 70% of the public guessing angry, the rest being disturbed, aggravated and mad. Kimmy and Sandy guessed mad and Jack guessed angry. Pose 7 is the command shy with 60% of the public guessing shy and Kimmy and Sandy guessing shy. Jack guessed happy and the public, in addition, guessed content, happy, embarassed and bashful. Pose 8 is the command bored with 70% of the public guessing bored and Sandy guessing bored. Kimmy guessed sad and Jack saw his sister as mad. The remaining public responses were uninterested, resenting and disgusted.

Plate II is of the subject Jack. The commands for poses 1 through 8 were: happy, afraid, pain, sad, thinking, hate, guilty and puzzled. Jack's responses to the slides were: happy, afraid, pain, sad, thinking, mad, guilty and puzzled. Kimmy's responses were: happy, afraid, pain, sad, sad, mad, shy and thinking.

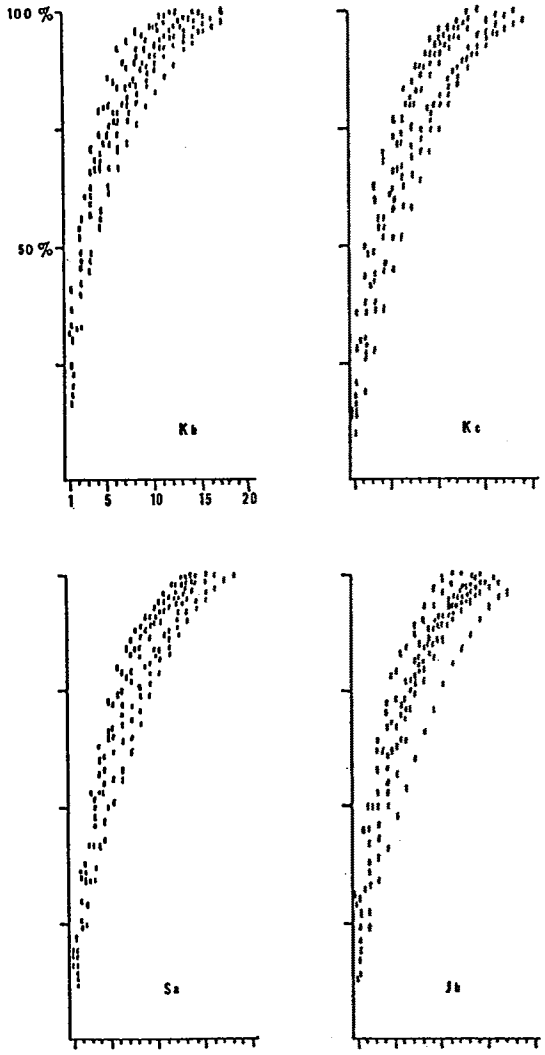


Figure 1. Graphs plotting number of words (abscissa) against accumulative percentage of agreement per word (ordinate). The agreement categories for each sample are plotted separately (Kb, Kc, Sa, and Jb). All the curves are so similar that no effort has been made to distinguish between the agreement categories on the graphs. The graphs show that the total amount of agreement, for whatever category, is determined by a small set of words, less than 20. The data for the graphs was taken from Tables 1, 2, 3, and 4.

1. 70% happy

2. 90% happy

3. 90% surprised

4. 80% pain

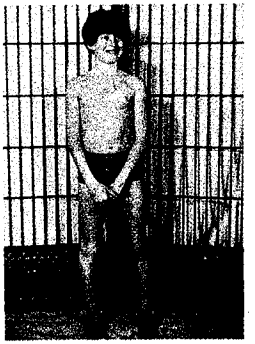
5. 60% thinking

6. 70% angry

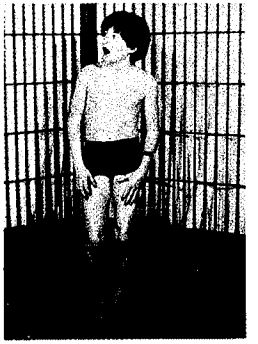
7. 60% shy

8. 70% bored

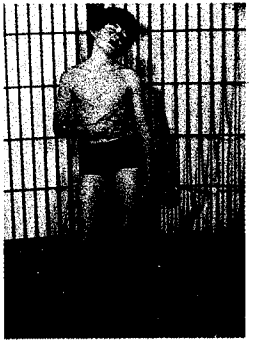
Plate I. Select sample of poses with public agreement 60% or better.



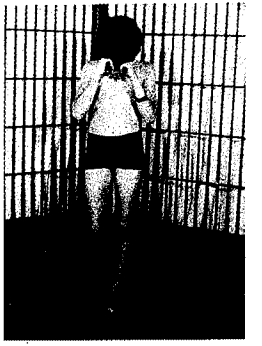
1. 90% happy



2. 80% surprised



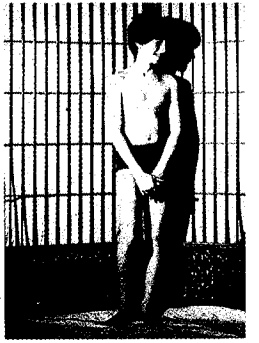
3. 70% pain



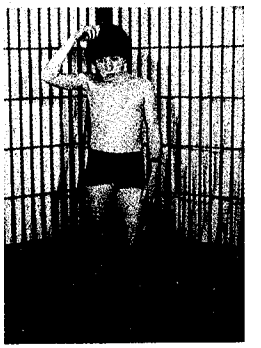
4. 70% sad

5. 60% thinking

6. 60% angry



7. 60% shy



8. 60% confused

Plate II. Select sample of poses with public agreement 60% or better.

Sandy said: happy, surprised, sick, unhappy, disappointed, mad, shy and thinking. The public agreement was: 90% happy, 80% surprised, 70% pain, 70% sad, 60% thinking, 60% angry, 60% shy and 60% confused.

Conclusions

The assumption that respondent variance in reading nonverbal communication is the product of synonymy has justified the reduction of disagreements by three methods: 1) words that mean the same thing can be collapsed into single categories, 2) respondents can be given a limited choice of responses, that is, denied free responses, or 3) responses can be scaled with semantic components, or universal dimensions of affect.

This work suggests that these strategies contain a fallacy. People use words to distinguish the differences they see. If they see a difference, a sememic contrast, they use a different lexeme. The choice of lexemes, i.e., the layman's 'words,' reflects the way the observers are compartmentalizing postural variability. Their distinctions reflect their impressions of discernability. Instead of trying to erase the disagreements, investigators ought to recognize it for what it is -- observers identifying the ambiguity of certain expressive poses.

This point might be focused using Pike's distinction between the etic approach and the emic approach (Pike, 1954). Pike makes the same point (p. 8): 'Yet in preparing this generalized but purely emic statement the minor characteristics of the individual emic systems must never be distorted or ignored in favor of a broad pattern of some kind ...'

The etic approach to body language involves the analysis of body physiology, that is, joint positions, balance, etc. (cf., Prost, 1974). The parameters of body physiology identify the various distinctions which categorize the etic members. The total array of all possible body postures creates the postural field. Thus, the postural field can and does in Pike's words (p. 8) classify 'systematically all comparable data, of all cultures in the world, into a single system...'

The emic approach (p. 8) discovers and describes a 'behavioral system with its structural units and structural classes of such units.' The emic postural units are the poses given in response to the individual commands. The emic response units are the words given after respondents observe the postures. The lexemes, or words, identify the contrastive observational units. The assumption behind synonymy is that the lexemes, or response words, are really part of an etic structure, their presumed 'meanings' constituting the emic levels. The data presented here is hard to interpret in any way other than to presume that the lexemes identify the respondents' distinctions and,therefore, the respondents' emic units.

A word of caution must be raised. This work dealt with posture only. From Birdwhistell's kinesic studies (1970) we know that people move and gesture and do so in various social contexts. In context, through gestures, through periods of interaction, subjects can communicate far more clearly than would be implied by the respondent agreements found here. Our research objective was to control the context and flow of interaction by restricting the observers to a simplified response structure dealing with postures only. Whether this strategy achieved its goal is not altogether clear. The respondents could have substituted, in their imaginations, what we tried specifically to exclude. The experimental situation itself may have introduced factors of which we were unaware. Nonetheless, the work was bluntly exploratory and the data does

stand firmly in favor of an interpretation which can now be profitably subjected to more rigorous scrutiny.

For about one hundred years, since the time of Darwin's work on expressive emotions in men and animals, researchers have been frustrated by peoples' inability to be thorough and concise in their interpretations of body language, but perhaps this _is_ the way people handle the world. The overall array of body postures is a continuous field of phenomena, intergradations abounding; the human mind brings order to this continuum by arbitrary compartmentalization. The way humans create the boundaries between the compartments is to leave a gap between the groups. These gaps are those poses on which observers disagree. Observers have less difficulty identifying the centers of the compartments and to these centers they apply a truncated set of words from their vocabularies. When observers see boundary cases they do their best and say so, with words different from those of their truncated sets. The social need is to be precise about the centers, those few states which have to be communicated unambiguously. For the rest there are no rules, no neat borders, just a welter of poses on which no one can agree.

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Éléments de l'encodage dans les langues naturelles

János BUTTLER
Bâle
- Suisse -

Les phénomènes de la langue refusent de se prêter comme objet d' une science inductive qui se fond uniquement sur l'observation des faits. Toute forme linguistique que nous touchons se révéle abstraction, car étant répétitive il lui manque l'individualité concrète. Ce qui se laisse répéter s'avère independant des déterminismes spacio-temporels et situationnels. Bien sûr, les observations fondées sur des faits dans les sciences proprement inductives sont aussi fixables, mais si nous ne nous laissons aller à la naïveté, il nous faut admettre que cela n' est possible qu'en vertu du langage dont nous nous servons comme d'instrument d'observation. Dans les faits propres du langage où nous ne pouvons plus recourir à un tel dispositif, il nous reste guère autre chose que de reconnaître d'emblée, la présence simultanée du concret et de l'abstrait.

Outre cette tension du concret et de l'abstrait nous en trouvons dans la langue une autre en correspondance étroite avec la première, celle du nombre fini des instruments que la langue nous met à disposition en face des besoins infinis de communication dont elle doit satisfaire en dépit de ses moyens très restreints.

Des antithèses de cette sorte - et on en trouve facilement d'autres - sont responsables en grande partie des difficultés que rencontrent les théoriciens de la langue. Grâce aux multiples efforts des théoriciens de reconcilier ces polarités, nous disposons aujourd'hui de formulations claires de quelques-uns de ces antipodes inhérents à la langue. On pourra compter ici des paires comme générale-particulier, institutionnel-spontané, collectif-individuel, fini-infini, continuité-discontinuité, structures-evolution, langue-parole et ainsi de suite. Une réconciliation naturelle de tous ces aspects contrariant l'éréction d'un édifice harmonieux théorique est aujourd'hui un désir insatisfait.

Il va de soi qu'un modèle fonctionnel de la langue doit incorporer organiquement ces tensions sans en abolir le dynamisme.

Pour arriver à ce modèle nous faudra-t-il, peut-être, changer complètement le point de départ. Voyons donc, ce qui arrive si nous inversons le problème et posons au centre même du fonctionnement de la langue, la source de ces déchirements et cessons de les voir comme provoqués du dehors. Une vue de cette sorte s'offre à nous si nous estimons que la langue opére simultanément en deux encodage différents. Ce serait l'issue d'une théorie du code hybride de la langue.

Nous nous proposons de donner ici une esquisse sur quelques éléments essentiels d'une telle théorie.

Un encodage hybride, c'est à dire l'accouplement de deux différentes techniques à une unité fonctionnelle nous est connu du domaine des ordinateurs électroniques. Cette classe des ordinateurs - notamment les ordinateurs hybrides - combinent les principes dits digitals et analogiques de la représentation symbolique. En dépit de ce fait, il ne s'agit pas quand à notre proposition d'une adaptation d'un modèle technique aux besoin de la linguistique.C'est plutôt la logique interne des propres faits de la langue qui nous a conduits à proposer le modèle à l'encodage hybride, et le fait que quelque chose de pareil existe déjà en matière technique n'est qu'une preuve en faveur de la viabilité du modèle.

En supposant la nature et le fonctionnement de l'encodage analogique et digital connus, nous nous contentons de noter que les caractéristiques antagoniques des deux techniques les plus importantes de notre point de vue sont la nature discontinue, finie, inprédictive, immotivée et arbitraire de la technique digitale face à la continuité, du nombre infini des états possibles, de la prédictabilité et de la motivation dans l'analogie.

La distribution des deux techniques à l'égard des fonctions partielles au sein de la fonction intégrale de la langue, selon notre vue, est telle que l'encodage digitale fournit, pour ainsi dire, la matière première des messages dans lesquels la langue se manifeste pratiquement et il revient à l'analogie de former les messages à partir de cette matière. Evidemment, nous avons à fair ici à la tension qui se manifeste entre les deux pôles de l'individuel et du collectif. Le jeu de l'analogie dépend, au moins dans quelques uns de ses aspects multiples, de l'activité individuelle du sujet parlant laquelle est d'une détermination spacio-temporelle unique mais puisant sa matière dans la propriété collective. Il faut tout de suite noter que cette répartition des rôles est encore très inexacte et n'a de justification qu'à titre préliminaire. Nous espérons apporter au cours de notre exposition plus de clarté en cette matière.

Aussi faut-il noter que l'activité du sujet parlant (ou écrivant ou pensant) n'est pas d'emblée régie par l'analogie. La première phase de la formation d'un message comprend donc le choix de ces unités qui vont entrer dans le message à émettre. Ce choix, nous voulons le comprendre cette fois comme déterminé par une motivation plus vaste n'entrant pas dans les cadres propres de la linguistique. Etant donné les unités choisies, l'analogie peut entrer en jeu pour produir finalement un message individuel adapté aux conditions uniques de telle et telle situation de discours concret.

Ce sont des cadres, encore trop vagues, où il nous faut localiser les éléments matèriels, formels et dynamiques de la fonction intégrale linguistique. Nous savons que la façon la plus persuasive de déployer ces éléments serait de suivre la voie de l'analyse, partant des faits particuliers et d'arriver par des généralisations graduelles à des théorèmes. C'était, bien sûr, la voie qui nous a conduits à nos conclusions mais pour adapter nos besoins présents à une esquisse très serrée, il nous semble que la voie de déduction et de la synthèse, partant des notions les plus générales nous permettra une exposition plus claire. (Pour une présentation plus detailée, nous renvoyons le lecteur au travail en langue allemande de l'auteur portant le titre - Versuch zur Hybrid-Kode Theorie der Sprache - Beiträge zur Theorie der Textinterpretation; Thèse, Université de Bâle 1974.)

Les investigations sur la nature du code digital, qui sert de base à toute activité linguistique nous conduisent à la problématique la plus profonde et en même temps la plus primitive de la symbolisation en général.

L'explication du fonctionnement d'un code se fait d'habitude de la façon suivante: on présente une liste des membres du code, ce qui est en principe une tâche simple car tout code digital comprend des membres en nombre fini, et on établit une-à-une les correspondances avec ce qu'elles signifient. Ce procédé, si on y regarde de plus près, perd tout de suite sa valeur explicative et s'avére une simple transcription. C'est à dire, les moyens par lesquels nous arrivons à ordonner à chaque membre son signifié, sont les produits d'un encodage préliminaire et nous renvoient proprement dit à cet autre code. Cet autre code doit être, certainement, plus large que celui qu'il tient à expliquer, car il doit couvrir non seulement les membres de celui-ci avec leurs correspondances référentielles, mais aussi la situation de l'explication, sinon, de tous ses emplois possibles. Sauf, naturellement, si - en suivant Wittgenstein - on prend recours à la démonstration physique. Mais, cette explication il nous faut la rejeter pour plusieurs raisons. D'abord, une telle démonstration est limitée à des objets qui sont démontrables au sens matèriel et par nature nettement délimités, ce qui ne serait encore qu'un obstacle purement pratique. L'objection de principe est que la démonstration physique ne se prête qu'à l'encodage d'objets individuels, c'est à dire, revient à la distribution de noms propres. Sûrement, s'agit-il dans ce cas aussi d'une fonction de code, mais excluant toute possibilité de généralisation. Il faut aussi voir, (et c'est une raison suffisante en soi pour ne pas nous contanter de l'explication démonstrative,) qu'une démonstration physique implique une situation préliminairement structurée par l'intervention d'un code quelconque. Nous constatons donc que sur ces voies d'explication, nous nous engageons inévitablement dans une régression sans fin. En nous trouvant en face des codes linguistiques fonctionnant en dépit de cette absurdité, nous nous voyons contraints de rejeter toute explication appartenant à cet ordre d'idée. La solution dans cette situation est de postuler, et cela nous parait sortir d'une nécessité a priori, l'existence d'un encodage primitif qui assure en lui-même la correspondance du référant et du référé. Sans doute, seules les langues dites naturelles remplissent ces conditions et c'est de celles-ci que dépendent tout autre code d'une valeur nécessairement plus restreinte. Nous analyserons brièvement les conditions dans lesquelles un encodage primitif réussit à surmonter les difficultés mentionnées ci-dessus. A titre de brièveté, nous n'élaborerons que quelques points centraux évidents en soi.

La difficulté principale qui nous paraissait insoluble par la voie de l'explication verbale, consistait dans la dualité du référant et du référé. C'est la même difficulté qui se manifeste dans un contexte plus vaste mais masquée par des vestiges historiques, quant au fameux cercle herméneutique. La solution de cette difficulté consiste, selon notre vue, dans les codes primitifs, du fait que ces codes ne connaissent simplement pas cette dualité. Comment comprendre cela?

Un code digital a par nécessité des membres en nombre fini. Cela implique qu'un tel code ne peut manier et connaître qu'autant de distinctions qu'il en a de membres. Si l'on parlait par rapport à un code d'un plus grand nombre de distinctions que le nombre de ses membres, cela ne saurait être légitime que d'un point de vue extérieur au code, c'est à dire, dans les termes d'un code plus ample.Mais occupant une telle position, nous nous engageons dans une régression, et c'est justement ce que nous voulons éviter. Si nous nous mettons dans la positions d'un observateur extérieur, il va de soi que nous disposons d'une capacité différentielle dépassant celle du code dont nous somme l'observateur. Alors, en comparant le code avec la situation nécessairement plus ample, les membres du code entreront dans des relations multiples à l'égard des éléments de la situation. Cela revient à une abstraction consistant dans l'incapacité à saisir certaines différences individuelles. Le niveau sur lequel cette incapacité se manifeste, ce qui est le niveau d'abstraction du code, est déterminé par le nombre de ses membres et le code n'est pas en mesure d'évaluer ce degré relatif à d'autres plans d'encodage possible. Ce n'est qu'en tant qu'observateur extérieur qu'il est possible de constater ce fait d'abstraction, car le code ne dispose pas de moyens pouvant enrégistrer ce fait. Pour cette raison, il nous est permis de formuler comme théorème:

un code digital primitif constitué par des membres en nombres finis et à quoi il peut se référer se trouvent au même niveau d'abstraction (1)

Si nous faisons disparaître le point de vue de l'observateur et son code plus vaste, nous ne nous engageons pas dans une régression mais nous cherchons à nous adapter à l'optique interne du code primaire, il nous faudra donc reconnaître que dans un encodage primitif les structures du référant et du référé sont isomorphes. Il n'y a aucune raison à distinguer entre les deux. Un code ne peut pas saisir quelque chose de plus différencié que lui-même, et par conséquent ne pas en être l'indice. La seule exigeance, à laquelle il doit satisfaire, est de différencier univoquement ses membres. Nous pouvons donc formuler comme théorème 2:

un code digital et son champ de référence sont isomorphes (2)

En tenant compte des conséquences qui sortent de l'évitement de toute régression à des métacodes, il faut aussi constater qu'un code ne dispose pas de moyens par lesquels il lui serait possible de délimiter sa propre compétence.Cela revient à dire qu'un code est fini et en même temps sans limite imposée par des facteurs extérieurs. C'est pourquoi:

un code digital primitif constitue une totalité (hors de quoi il n'y a pas d'existence pour lui) (3)

Suivant cet ordre d'idée, comme le code ne peut subir aucune restriction externe, il est évident que cela est aussi valable par rapport à sa structure interne. C'est à dire, les membres du code sont nécessairement contigus, car il lui manque tout moyen d'introduire des restrictions à cet égard. Par conséquent:

un code primitif, ainsi que son domaine de référence isomorphe, constituent un champ complet structuré par leurs membres contigus et bien delimités (4)

La capacité de différentiation du code a evolué exactement dans la mesure où celui-ci peut différencier ses membres. Pour assurer cette différentiation multiple, les membres du code doivent être composés. Seulement un membre d'un code constitué par une seule unité pourrait être unitaire. La complexité des membres doit reposer sur des termes permettant la confrontation des membres. Si aucune comparaison des membres du code n'était possible, si chacun de ses membres était une individualité incommensurable, on ne pourrait pas distinguer un nombre fini d'unités car on ne saurait jamais ou il s'agit d'une variation graduelle et ou il s'agit d'une différence pertinente. Une distinction univoque n'est possible que sur la base de contrastes oppositionnels. Ce fait se laisse formuler:

les membres d'un code primitif sont des unités discrètes qui se différencient selon des contrastes oppositionnels (5)

Un corollaire de cette théorème dit que:

la complexité des membres d'un code est proportionnelle au nombre des membres qui le constituent (6)

Cette esquisse sur les conditions de l'encodage primitif est, bien sûr, une construction théorique et rationnelle, manquant toute persuasion immédiate. Une définition de la fonction d'un code digital primitif - comme p.e. l'essaide Wittgenstein - doit échoir, car comme nous l'avons vu, ou bien elle se fait coupable d'une régression infinie ou bien devra se rabattre au niveau des existences particulières.

La difficulté consiste dans le fait que nous ne pouvons jamais arriver à la contemplation directe d'un code digital primitif. En choisissant une de ses unités et si ce n'est qu'à titre d'observation, nous l'engagerons immédiatement dans une situation, dans un procédé de concrétisation aussi latent qu'il soit et nous l'avons contaminé de fait individuels appartenant à la réalisation des messages. Pour la saisir dans sa pureté native, nous sommes renvoyés à la reconstruction intellectuelle en la dégageant des éléments contingents qui l'enrichissaient au cours de la réalisation psychique. Le code digital est, pour ainsi dire, rien de plus qu'un point de repère à partir duquel nous nous orientons et sur lequel nous ne pouvons jamais jeter un coup d'oeil dans son abstraction propre, mais sans lequel nous ne saurions jamais reconnaître des idées distinctes et identiques dans nos procédés psychiques.

Les actes de communication font part des situations concrètes. Là, ils mettent à jour une variété infinie. Et comme il n'y a pas deux situations de communication absolument identiques il n'y a pas non plus deux messages absolument identiques. Pourtant, le contenu répétitif d'un message qui peut être retrouvé dans une infinité de situations et dans des messages acoustiquement très différents est reconnu comme tel par chaque membre d'une communauté de communication. Cette capacité est elle-même, d'ailleurs, un des critères de l'appartenance à une communauté linguistique. De cette façon, il nous est possible de distinguer entre le contenu, le 'quoi' et entre le mode d'apparition de celui-ci, le 'comment' d'un message. En d'autres termes, c'est avec la relation de l'invariant aux variants ou bien celle du génotype aux phénotypes que nous avons à faire ici. La réussite de cette distinction est la condition absolument première de l'établissement d'une science autonome de la langue. Elle est tellement fondamentale que la linguistique n'en a même pas fait l'objet de sa réflexion. Elle la présuppose tacitement comme garantie. Cette garantie lui parait être dans une telle mesure spontanée qu'il n'existe même pas de terminologie à refléter cette distinction. Apparament, on peut s'en passer sans provoquer la critique. Le consensus à cet égard est accompli. Pratiquement, on se réfère au 'quoi' comme contenu cognitif, conceptuel ou intellectuel,ce qui montre que pour le saisir on ressort à des critères externes, gnoseologiques, logiques et même psychologiques. Le domaine de la variation, de la région des phénotypes manque même d'une étiquette sommaire. Pour combler cette lacune, nous couvrirons provisoirement ce domaine par le terme de 'style'. C'est le 'comment' opposé au 'quoi' des messages.

Comme solution défintive de ces embarras terminologiques, une seule pourrait être acceptée ayant alors un fondement linguistique interne. Des critères propres à la semiologie ne faisant pas appel à des disciplines étrangères, nous sont fournies par les techniques d'encodage. Correspondant aux idées développées précédement, nous allons associer la technique digitale avec le 'quoi' et la technique analogique avec le 'comment' des messages. En faisant ainsi, nous nous procurons des critères purement formels pour la classification future des faits du langage.

La nécessité de la construction des messages dérive du fait que le code digital, ayant un inventaire très restreint par rapport aux exigences, ne permet pas aux sujets parlants d'arriver à leur but, en choisissant uniquement un membre du code. Ils doivent donc, selon le principe du 'do-it-yourself' fabriquer eux-mêmes ce dont ils ont besoin dans la communication. Cette activité des sujets - en laissant les actes de choix du côté - est sujette au principe de l'encodage analogique.

C'est ainsi que l'analogie est appelée à médiatiser entre moyens finis et besoins infinis, entre abstraction propre au code digital et la nature concrète des messages actuels, entre le bien collectif rigide et l'individualité spontanée de la parole.

Afin de voir clairement l'analogie en oeuvre il est indispensable d'identi-

fier le substrat qui lui sert de base d'opération. C'est le code digital qui nous permet cette identification, il est, pour cela, le premier élément du modèle fonctionnel de la langue.

Quant à l'analogie, elle renferme une relation au moins à deux termes: quelque chose est analogue par rapport à quelque chose d'autre. En matière de communication, il s'agit naturellement du rapport du référant au référé, du vehicule du message à son contenu ou inversement. Au niveau purement digital d'un code primitif, nous avons dû conclure qu'une distinction entre signification et son véhicule, n'est ni legitime ni possible. En faisant sortir des unités du code digital, en les écartant du niveau d'abstraction propre à ce code, cette distinction gagnera d'autant plus de bon sens que nous nous éloignerons dans le sens du concret. Comme stade final, dans la réalisation accomplie et symétrique, nous arrivons d'un part à un signifiant, un signal physique, sensible, ephémère et unique: la parole concrète, d'autre part à son corrélat signifié comme sens concret, expérience psychique, représentation individuelle et personnelle. Entre les deux, la relation de l'analogie se tient à travers toutes les phases de l'épanouissement réalisatrice. Le garant essentiel de leur correspondance reste tout de même le fait qu'ils sont tous les deux, pour ainsi dire, suspendus au même dispositif, le code digital. Cette dépendance du code digital permet tant à l'émetteur qu'au récépteur de se référer par la réduction abstractive aux mêmes points fixés par l'usage collectif.

Il nous faut voir comment ce domaine très hétérogène, occupant l'espace entre l'abstraction du code et la concrétisation des messages actuels se laisse structurer et médiatiser sous le principe de l'analogie.

La sélection de tel et tel membre du code selon l'intention communicative du sujet n'ajoute dorénavant rien de matériel à ceux-ci. Ce qui s'ajoute dès qu'une sélection est accomplie, c'est la séquence spécifique des membres choisis dans l'ordre linéaire de la parole à réaliser. Cet ordre est évidemment un résultat de l'intervention du sujet effectuant la sélection. L'ordre, que celui-ci introduit dans la matière choisie, permet déjà la mise en valeur du principe analogique. Un ordre quelconque, une disposition séquentielle d'un groupe peut ressembler à quelque chose si ce n'est qu'à une autre séquence. Cette première mise en valeur de l'analogie,nous la désignons comme formelle.

L'analogie formelle, en permettant aussi une certaine ressemblance entre signal et signifiance, permet non seulement de conclure du premier au deuxième et réciproquement (nature prédictive) mais aussi à la ressemblance des signifiés de messages ressemblants et vice versa. Nous noterons que par ces possibilités inhérentes à l'analogie nous avons raison déjà à distinguer entre le signal et son signifié, c'est à dire, nous abandonnons par ce fait le domaine propre de l'isomorphisme. Bien entendu, cela ce passe en n'ajoutant rien, au sens matériel, aux membres du code digital que nous avons choisis. L'enrichissement matériel de ceux-ci n'aura lieu que dans la phase poussée de la réalisation ou le vehicule sensuel fait son apparition. Cette

phase de la réalisation ne serait atteinte dans la communication orale qu'au niveau phonétique: dans la communication graphique, du type alphabetique, au niveau phonologique, du type syllabique au niveau de ce que nous pouvons désigner avec une certaine imprécision le niveau morphologique et ainsi de suite.

L'enrichissement matériel de la signification reste thème de la description et de l'analyse phénoménologique de l'expérience introspective, relative aux procédés mentaux concomitants.

Voyons maintenant ce qui peut résulter de la première intervention du sujet, en tant qu'il effectue une séquence des unités du code qu'il a sélectionnées. Nous ne voulons pas toucher ici au problème de la compatibilité des unités du code, ni aux restrictions qui se posent quant aux choix successifs. La discussion de cette matière présuppose une analyse de la structure interne du code ce qui nous amenerait à faire des digressions. Nous présupposons que les unités choisies sont susceptibles d'entrer dans des combinaisons voulues.

La disposition la plus simple des unités sélectionnées est la juxtaposition dans une séquence dont tous les membres sont équivalents. C'est la coordination qui représente la combinaison la moins organique possible. Comme énumération, elle peut, bien sûr, reproduire analogiquement un ordre quelconque, mais à part de cette ressemblance il n'arrive aux membres coordonnés rien de plus que ce qui arrive à une seule unité choisie (c.à.d. la réalisation amorce immédiatement la phase phonique). De cette façon, la détermination de leur rôle dans le message ne pourrait être préciser qu'au cours des pas suivants de la réalisation. La juxtaposition simple reste d'ailleurs interprétable au niveau du code. Si nous comprenons le code comme un champ de structuration contiguë et discrète, l'addition de quelques-uns de ses membres ne donne pas naissance à de nouvelles lignes de délimitation et leur mise en relief n'altère pas les distinctions préexistentes dans le code.

Une combinaison organique des unités sélectionnées est effectuée par leur intersection. L'intersection, évidemment, ajoute des délimitations jusqu'ici inconnues au code et pourrait être interprétée comme un raffinement de sa structure. En tant que les éléments qui définissent les nouvelles délimitations proviennent du code lui-même (c'est à dire, elles résultent de l'application interne de ses propres critères de différenciation), le nouveau dessin peut encore être interprété au sein du code d'une manière satisfaisante.

Du point de vue du sujet intervenant, la chose n'est tout de même pas si simple. L'ordre linéaire des unités se couvrant partiellement lui posent des problèmes de priorité. C'est à dire, s'il ne s'agit pas de la pure juxtaposition, ou tout les membres de la séquence sont équivalents, il lui faut décider quant à la valeur relative de ceux-ci. Ce problème est connu dans la grammaire comme celui de la subordination. L'analyse des relations de subordination du point de vue formel a son corollaire exact du côté du signifié. A l'intersection, qui peut être interprété d'une manière satisfaisante comme un nouveau compartiment dans le code, s'ajoute le point de vue du sujet qui effectue des relations de subordination. Cela détermine la répartition des rôles que les

grammairiens désignent par une terminologie heureuse d'une part de fonction d'identification ou de déterminé d'autre part de fonction de différentiation, de sélection ou de déterminante. Cela revien à décider laquelle des unités va être considerée comme livrée à la réstriction par l'autre: un linguiste révolutionnaire n'est pas un révolutionnaire linguiste. Mais ce fait là ne peut plus être interprété au niveau du code. Voilà comment un moment analogique nous contraint à sortir du code et permet en même temps au signifiant et au signifié de s'écarter l'un de l'autre. Nous voulons réserver le terme syntagmatique à ce domaine de la combinaison où plusieurs unités forment par restriction mutuelle, une nouvelle unité interprétable au niveau du code.Notons ici, en passant, la problématique des cases vides, des lacunes systématiques du vocabulaire.

Avoir un sens au sein du code, cela veut dire que le raffinement virtuel du code revient à l'actualisation d'une potentialité inhérente. Cette potentialité devient actualité permanente, si la combinaison se fixe et entre dans l'inventaire. Par contre, la hiérarchisation subordinative de la séquence syntagmatique reste une affaire relative à la réalisation ne trouvant aucune interprétation dans le code. A part cela, les syntagmes ne sont pas autres choses que des pseudo-unités fabriquées suivant les nécessités momentanés de la communication. Il nous faut ajouter que - dans beaucoup de cas - la mise en relief de la subordination, ce qui est la distribution des rôles d'identification et de différenciation n'est pas mise en exécution par des moyens purement morphologiques et séquentiels et des instruments prosodiques y peuvent intervenir. Ces derniers relèvent de la matière phonologique et phonétique et là, nous nous trouvons poussés davantage vers le pôle opposé de la réalisation, le signal concret.

Une troisième espèce de liaison - outre la coordination et la subordination - est la relation prédicative. Celle-ci ne peut consister que de deux membres: sujet et prédicat, thème et rhème etc. Qu'est ce qui se cache derrière cette relation tant soignée par la grammaire et la logique traditionnelles? Si nous avons pu identifier la relation de coordination comme addition - symbolisée par '+' - les relations syntagmatiques subordinatives comme détermination réstrictives - symbolisées par '-' - il nous faut donc reconnaître dans la relation prédicative l'identification pure, - symbolisée par '='. En d'autres termes les deux membres de la relation prédicative - soient simples soient syntagmes complexes - se couvrent exactement.

Cette équation qui dénonce l'identité de deux régions du code ne pourrait être interprétée sur son plan que comme la synonymie des deux expressions en cause. Certainement, ce n'est pas cela qu'un énoncé prédicatif vise à communiquer. Afin de lui attribuer un sens plus substantiel, il nous faudra abandonner le champ du code et envisager le plan de la réalisation. Là, l' équation de deux syntagmes (ou de deux unités simples) trouve une interprétation naturelle: elle établit la validité de deux aspects différents à l'égard d'un objet ou d'un fait en général exterieur au code. En d'autres termes, la construction d'une équation prédicative revient à la présupposition d'un plan extérieur ou bien à la construction projective d'une réalité à interprétations multiples à partir du code. Bien sûr, pour trouver cette interprétation, il ne nous faudra pas dans tous les

cas, pousser la réalisation jusqu'au niveau concret.

C'est la quantification des deux membres de l'expression prédicative qui est appelée à déterminer la mesure de la réalisation. On réalisera, selon le cas, non seulement le concret mais aussi des degrés différents de généralisation indépendamment l'un de l'autre, dans le sujet et dans le prédicat. C'est justement ce en quoi consiste l'usage de la langue comme instrument de la pensée productive: l'établissement des relations entre des faits plus ou moins concrets ou uniques et entre des généralisations tirées soit du code même soit des énoncés prédicatifs antérieurs. Tout raisonnement scientifique est une texture composée alternativement de valeurs générales et particulières.

En disposant du code et des mécanismes appelés à engendrer des syntagmes et des équations prédicatives, nous sommes donc en mesure de construire tout un univers. Nous reconnaîssons dans cette possibilité l'ambition suprême de l'esprit rationaliste. La vanité futile de cette prétention sera reléguée dans les confins d'un jeu métaphysique par le contrôle subi au cours de la réalisation. Là, seront jugées la possibilité et l'acceptabilité des combinaisons des unités du code, à moins qu'il ne s'agisse d'expressions analytiques.

La quantification permet donc la diversification des membres du code (et des syntagmes construits de ses membres) dans le monde empirique extracodal, en tant que leur usage peut embrasser un nombre non limité de faits et d'objets. L'unique condition de leur application est la classification adéquate des faits envisagés dans les catégories offertes par le code.

A l'encontre de cette diversification, se présente l'autre en sens inverse: l'identification prédicative laisse apparaître, dans le code, la même chose ou les mêmes événements dans diverses perspectives. Les deux, ou, à volonté, plusieurs faces d'un et du même objet se reflétant dans le code sont comme la détermination trigonométrique d'un point sur un plan à l'aide d'au moins deux autres points. Cette diversification qui apporte des interprétations multiples catégorielles aux faits extracodals aboutit en fin de compte à l'interprétation cohérente du monde vécu. En quoi pourrait donc consister la connaissance de quelque chose de nouveau si ce n'est dans la mise en connection avec le réseau des interprétations déjà établies?

Remarquons que l'interprétation des quantificateurs, soient singuliers, soient numériques, soient indéfinis, nous indique que la nature de toute opération arithmétique est secondaire à la réalisation. Nous trouvons dans ce fait une confirmation des vues de Charles Bally qui reléguait le concept du nombre au domaine de la parole. En admettant que par la quantification, il nous est possible d'envisager des groupements pratiquement infinis de la réalité extracodale, il reste tout de même que nous n'en saisissons toujours que des vues de restriction catégorielles. La nature finie du code nous offre une discontinuité des qualités que nous attribuons à nos expériences. La construction des syntagmes nous permet, certainement, d'introduire un échelonnement plus nuancé mais non pas l'abolissement des structures qui sont par principe discontinues. La combinaison des entités d'un inventaire fini ne dépassera les structures

disontinues qu'au prix de l'engendrement d'ensembles sans limite. Des ensembles de dimensions illimitées présupposent, de leur part, l'occurence récursive soient de leurs relations soient de leurs membres constitutifs. L'occurrence récursive, cependant, détruit la hiérarchie de subordination, caractère essentiel de tout syntagme. La subordination cède alors à la coordination et il en résulte la juxtaposition anorganique. C'est là, une conséquence logique de notre constatation précédente, selon laquelle le dénombrement relève de la réalisation et non du code digital. De toutes façons, des structures et des entités récursives ne sauront être différenciées au sein du même ensemble que par des indices numériques.

La productivité des constructions syntagmatiques et de leurs liaisons prédicatives est donc sujette à une limitation imposée par le volume du code. Les moyens de la combinaison séquentielle ne suffisent donc pas aux besoins de la communication concrète d'une variété infinie. Pour arriver à ce but, il faudra dépasser les instruments de la combinaisons linéaire et de l'analogie formelle qui s'y manifeste et recourir à l'analogie matérielle relèvant de la substance phonique. Par l'adaptabilité de celle-ci à chaque acte de communication concret, les messages obtiennent la possibilité d'entrer dans des relations une-a-une avec des référés d'une détermination spacio-temporelle unique.

Les phases successives de la réalisation par l'analogie matérielle pourront être suivies selon deux lignes principales: l'une concerne les faits prosodiques et nous conduit des structures logiques des énoncés, aux modalités psychologiques, l'autre à travers les diverses manifestations de la symbolisation sonore aux extrèmes des onomatopées. Nous arrivons ainsi au point où une investigation inductive aurait dû être amorcée. Nous sommes arrivés aux énoncés phénotypiques d'où, en sens inverse, nous pourrons remonter par les échelles de l'abstraction au formalisme pur du code digital.

La distinction des deux phases de la réalisation correspondant à la distinction entre analogie formelle et matérielle nous aide à une vue plus nuancée, quant au problème de la classification des faits de la syntaxe traditionnelle. La syntaxe relève-t-elle de la langue ou de la parole? Nous savons que cette question, remontant à la pensée saussurienne, n'a rien perdu de son actualité. Bien au contraire, elle en a gagné au cours des dernières années. Du point de vue de l'encodage hybride l'alternative: syntaxe, langue ou parole, nous parait un peu simpliste. D'abord, l'engendrement des séquences linéaires, à partir des unités séléctionnées du code digital se divise clairement en deux sortes d'opérations distinctes: la syntagmatique et la syntaxe propre. La première effectue par la combinaison de quelques unités compatibles une structure qui n'existe pas dans le code mais y trouve une interprétation sensée. Les syntagmes pourront par conséquent être incorporés dans le code, en raffinant le réseau préexistant de celui-ci. Si le syntagme se fixe, il a bonne chance de devenir, en se démotivant, un membre de l'inventaire et pourra être choisi comme entité unitaire pour servir à la communication. La combinaison n'aura plus de rôle à son égard. Les expressions dites idiomatiques soulèvent ici une

problématique extrêmement complexe qu'on évitera même d'effleurer dans une esquisse trop hâtive. Nous noterons seulement le caractère double des expressions idiomatiques, en tant qu'elles nous semblent garder leurs motivation, malgré l'encodage unitaire.

La présence du sujet parlant par rapport aux syntagmes ne se manifeste que par des indices très discrets: la hiérarchie de subordination nous renvoie à une perspective dépassant le code. La liaison prédicative des deux syntagmes - la syntaxe au sens restreint - ne trouve plus une interprétation satisfaisante dans le code et nous renvoie définitivement aux réalités externes. De sa part, l'expression prédicative pourrait certainement aussi subir l'incorporation dans le code, mais cela ne pourrait arriver que par la voie d'une transformation en syntagme. Evidemment, par cette transformation, nous abolissons la contingence des faits extérieurs.

Le fait que ni la subordination, ni la prédication ne sont concevables sans les dimensions extracodales et subjectives nous prouve que la syntaxe et, dans un degré plus atténué, aussi la syntagmatique relèvent de l'individuel et de l'occasionnel. Comme tels, il nous est interdit de les classifier sans restriction comme appartenant à la langue, institution établie par le collectif. D'autre part, la susceptibilité des structures syntagmatiques et syntaxiques d'être incorporées dans le code digital et le fait que leur nombre est par principe limité, suffisent à découvrir la régularisation institutionalisée qui s'oppose à la concrétisation individuelle ultime. Nous constatons donc dans le domaine de l'analogie formelle, un enchevêtrement des faits de langue et des faits de parole suivant la terminologie saussurienne. C'est ici une phase de transition qui apaise la tension insoluble entre les deux pôles, celui de l'autorité dictatoriale de l'institution sociale et celui de l'anarchie des particularités individuelles.

L'incorporation des syntagmes dans le code et leur démotivation concomittante équivalent à la perte de leur caractère synthétique. Les membres du code apparaissent en entités unitaires, leur sélection est faite par un unique acte de choix. Cette nature unitaire ne doit tout de même pas être comprise dans le sens absolu. Elle est relative et n'est valable qu'en face des syntagmes. Un des théorèmes du code digitale primitif établissait aussi le caractère complexe de ses membres, sans lequel ils ne pourraient pas se différencier. Cette complexité des membres du code ne servant que la distinction, n'intéresse plus les opérations de réalisation. Du point de vue des structures, c'est donc une décision arbitraire de reconnaître une complexité comme syntagme, une autre comme membre du code. La légitimation de cette décision est de l'ordre fonctionnel et opératoire. Une unité comme membre d'un ensemble tient sa valeur en vertu de la présence simultanée de l'inventaire entier.

Cette simultanéité, nous pouvons l'associer - sous quelque réserve - à l'ordre paradigmatique de Saussure. Les syntagmes et les liaisons prédicatives, par contre, représentent une succession linéaire temporelle: une suite d'événements sujette aux lois de la probabilité statistiques. Les procédés d'in-

formation objectifs et de compréhension subjectifs, relatifs à la transmission des messages sont essentiellement réductibles à ce caractère de séries statistiques.

Il faut comprendre la linéarité des messages en fonction de la dimension du temps. L'engendrement et la transmission des messages ne peut se porter que dans ce milieu. Parallèlement à la séquence temporelle que requiert le message, il nous faut aussi postuler une fonction d'intégration pour assurer, dès qu'un message minime est conclu, son saisissement synoptique. Un aspect de cette vue intégrante est justement la possibilité de l'encodage unitaire. Ce sort est bien exceptionnel, les messages tombent ordinairement dans l'oubli pour renaître de nouveau en cas de nécessité. Mais les structures simultanées qu'engendrent en fin de compte les messages linéaires ne ressemblent en rien à celles de leur origine première, du code. Les elements analogiques surajoutés au cours de la phase linéaire, la motivation, restent témoins de la réalisation, de l'adaptation à une réalité hors du code.

Pour résumer, nous nous trouvons en face de trois structures très différentes: deux relèvant de l'ordre simultané et une qui les rattache: linéaire, successive. L'une des simultanées est analogique, motivée et manifeste pour cela un certain degré de concrétisation relative à l'autre qui est arbitraire, immotivée, abstraite. La première, clairement iconique, la deuxième, une espèce de matrice, topologie abstraite d'une structuration oppositionnelle.

Si nous cherchon le substrat de ces trois espèces de structures composant le procédé entier de la communication du type langue, nous retrouvons la phase linéaire dans les conduits nerveux rattachant les organes centraux et périphériques et dans le milieu transmissif du signal acoustique. Ensuite, nous sommes amenés à localiser le code digital dans le système nerveux central, en admettant que la façon dont il est enregistré échappe encore complètement à notre connaissance. Il y a bonne raison de croire que les recherches sur la physiologie du cerveau humain apporterons un jour quelque lumière sur ce point. Ces structures neurologiques ne sont certainement pas immuables mais en rapport aux messages transitoires, elles représentent une référence absolue achronique. Sa relativisation dialectique est un tours logique fallacieux.

Les structures simultanées produites par les messages, ont le caractère analogique, motivé et contrairement aux structures du code immédiatement accessible à la réflexion discursive et à l'intuition. Ces deux approches sont assez disparates, cependant il nous faut les admettre toutes les deux comme valables. Le fait que la somme integrée d'un message linéaire se prête à la réflexion discursive présuppose la répétition et ainsi un certain degré de fixation. D'autre part, par l'intuition, nous nous trouvons dans la dernière phase de la réalisation accomplie. Nous croyons que la nature iconique des messages peut expliquer le mieux la coexistence de ces aspects tellement divers.

Le caractère iconique des messages pourrait être décrit et interprété avec une tournure certainement simpliste et imprécise, comme la traduction analogique de la matrice abstraite qu'est le code digital. L'adaptation analogique assure

aux produits finaux des messages, une universalité, à l'encontre du code qui a une spécificité. C'est pourquoi une traduction interlinguale ne peut être accomplie qu'au niveau d'un message minime complet, de la phrase, et non des éléments constituants.

Etant donné que - du point de vue structural - le niveau de l'encodage digital est défini arbitrairement, il reste à la recherche empirique de le déterminer dans les cas concrets. Pour nos besoins, nous pourrons l'identifier en générale avec celui du lexique. En procédant ainsi, la difficulté n'est que transposée quant à la définition du mot. Les décisions prises à cet égard ne seront valables que pour telle ou telle langue particulière.

Les structures se trouvant au dessous de ce niveau exeptionnel, en tant qu'elles ne sont pas présentes pour la conscience linguistique naïve, pourront être caractérisées comme subgrammaticales. Leur importance et leur réalité deviennent manifestes, entre autre, quant aux aspects techniques relatifs à l'éxécution des procédés d'information. La graphie la plus pratique dans l'histoire, l'alphabétique, c'est p.e. établie au niveau phonologique. Nous connaissons, surtout grâce à la technique électronique, aussi bien des analyses dépassant de plusieurs degrés la résolution phonologique. Cela est valable tant au côté du référant que de celui-ci du référé. Suivant de tels indices, nous aboutissons finalement à la connaissance plus intime, mais toujours médiatisée, de la construction interne du code digital.

Les structures au-dessus du niveau de l'encodage digital sont sujettes au maniement conscient des parleurs. C'est le domaine des constructions motivées, relevant de l'analogie formelle, de dimensions très différentes. Que ceux-ci, de leur part, renferment aussi la possibilité de l'encodage digital et qu'ils représentent une réalité linguistique est demontré - entre autre - comme dans les structures subgrammaticales, par les divers systèmes d'écriture de l'ordre idéographique et pictural.

Ces plans d'encodages possibles contenant des unités d'une complexité croissante et gardant leurs traits analogique, ne saurons - tout de même - jamais être acceptés comme un code propre. Leur motivation et leur prédictivité traduisent des procédés plus ou moins avencés de la réalisation. Cela implique donc la présence d'éléments stylistiques, autre incompatibilité avec la nature digitale. En outre, au-dessus d'une certaine complexité, personne ne pensera plus à la possibilité d'un encodage effectif et on voudra bien définitivement abandonner le terrain aux analyses stylistiques et littéraires. Tout de même, on ne pourra pas nier que dans le domaine des formes littéraires, il est bien possible de déceler des conventions de la netteté des invariants génotypiques face aux exécutions variantes. Nous croyons que la pénétration linguistique de ces domaines se laissera toujours faire en fonction de l'ingénuosité linguistique et de la définition que la linguistique se prêtra à elle même.

VI

Approaches to the Description of Natural Languages

"PRETTY DAMN SELDOM...": ON THE GRAMMATICALITY OF UNGRAMMATICAL UTTERANCES

Valerie Becker Makkai

University of Illinois
at Chicago Circle

To some readers my title may be self-explanatory, but I would venture to guess that to most people it is a puzzle. The last half of the title seems to be a contradiction in terms and appears to bear little or no relation to the first half. Let me first address myself to this latter group of readers and ask them to consider the question of the grammaticality of my title. Is it, just as it stands, grammatical? Is it an acceptable English utterance? Most likely the answers would take some or all of the following forms: "Well, it depends on what you mean by grammatical---certainly it is somewhat odd semantically"; or "I assume that you will explain what you mean by it, so I guess it's OK"; or "I can't answer your question without more information"; or even "It may be grammatical in your dialect, but it's not in mine".

But suppose that I won't accept any of those answers. Suppose that I want, right now, a straight "yes" or "no" answer---no ifs, ands, or buts. Most linguists would probably be reluctant to commit themselves either way because they would feel that they don't have all the facts. But why do we need more facts? Isn't any utterance, on the face of it, immediately labelable as grammatical or ungrammatical? The answer to this latter question, I contend, must be "no". That is, there are circumstances, and actually many more than is commonly realized, where the grammaticality of an utterance is only apparent in a larger context---sometimes a linguistic context, sometimes even an extra-linguistic one. Thus when someone tries to pin us down to a grammaticality decision on an utterance taken out of context, we resist. Those readers who recognized the quote in my title no doubt have no problem in deciding on the grammaticality of it---it most likely appears quite intelligible and clearly grammatical. This, of course, is because they already know the context of the utterance and so don't have to wait to have it explained. This phenomenon has been discussed at length by Adam Makkai in various papers dealing with what he calls the Contextual Adjustability Principle, or *CAP*ping, for short.[1] Thus, depending on the supplying of the proper context, we might well find ourselves accepting as grammatical sentences such as the following:

Mary is greater than Bill and vice versa.
Susan is better than Martha, but Martha is gooder.
or even: Colorless green ideas sleep furiously.

I leave it to the reader's imagination to create suitable contexts for these sentences.

To get back to the quote in my title, let me not prolong the suspense any further. The quote is from a joke which appeared years ago in *The Reader's Digest* and which has been reprinted several times since by popular request. It goes something like this:

> *A Chinese businessman is travelling through the United States by train. He arrives in New York and finds that he has several hours' lay-over before his train is due to depart, so he decides to see a little of the city in the meantime. He checks his bag at the baggage room and leaves. About an hour before his scheduled departure he comes back and presents his claim check to the baggage master who disappears behind the racks of suitcases. After about fifteen minutes the baggage master returns to report that he is having trouble locating the bag and disappears again. Time drags on, with the Chinese businessman becoming more and more worried. The baggage master returns again, very apologetically, to say that he is still searching for the bag, and goes back to look some more. The businessman paces back and forth, nervously watching the clock. Finally about two minutes before train time the baggage master comes back to announce that the bag seems to have been lost. With that, the Chinese businessman completely loses his Oriental cool and bursts out:* "Pretty damn seldom where my bag go! She no fly! You no more fit run station than Godsake! That's all I hope!"

This joke, and quotes from it, have become proverbial in countless families. People recount how the tension arising from someone's angry remark can be completely dissolved by a suffixed "That's all I hope!". A prefixed "Pretty damn seldom" can soften the blow of whatever follows. Arguments can be abruptly terminated by an interjected "You no more fit run station than Godsake!". To those familiar with the joke there is no question of the grammaticality, to say nothing of the usefulness, of these utterances. They ostensibly were first uttered by a non-native speaker of English, their first occurrence certainly would be considered ungrammatical, and in fact their humor rests in their ungrammaticality. Yet they have been adopted by native speakers of standard English and are used in the midst of standard English utterances just the way any accepted native phrase would be. To the uninitiated they may sound ungrammatical, but to the initiated they are as grammatical as any other English phrase.

Their grammaticality is demonstrated by the fact that the syntactic patterns used in the joke have become productive. Thus one may hear utterances such as the following:

> Pretty damn seldom where my hat go!
> Pretty damn seldom what you do with the pen you borrow yesterday!
> Pretty damn seldom why my shirt have ring around the collar!
> You no more fit drive car than Godsake!
> You no more fit run conference than Godsake!

Note that it would be quite incorrect to say "Pretty damn seldom why *does* my shirt *have* ring around the collar", or "Pretty damn seldom where my hat *went*", or the like. After *pretty damn seldom* one is required to use a relative clause, not an interrogative, and all verbs must be in the present tense. Any other construction would be ungrammatical.

Now I hope that the foregoing discussion has raised in the reader's mind the question of what exactly is meant by the term "grammatical". It may seem that I have used the term rather loosely. That was intentional, because I want to confront the reader with the need to think about the term and the need for an adequate definition of the concept of grammaticality. There are certain utterances that we all would agree are grammatical, by whatever definition we give the term, and no doubt we would all concur on the ungrammaticality of other utterances. It is for this reason that linguists have been able to proceed for years, deciding on questions of grammaticality, without ever adequately defining the term. It is only when we are confronted with borderline issues that we become aware of the fact that we really have no adequate definition and that we are *not* all agreed on what we mean by it.

On the *surface* of it, if I may use that term, it might appear that grammaticality ought properly to be restricted to questions of syntax. But even if we allow this very restricted use of the term we get into difficulties, since linguists are not at all agreed on the question of just how much is encompassed by the term syntax. Actually there seems to be no justification for such a limited use of the concept of grammaticality. Yet in its broadest sense it could refer to all levels of language, from phonetics on up through semantics and perhaps even cognition. Certainly we talk about well-formedness on all these levels, and we might be led to assert that actually grammaticality and well-formedness are just two words for the same thing. But are they? Or to put it another way, is there not perhaps a need to distinguish between two types of acceptable structure---on the one hand those which are well-formed, and on the other hand ones which are not *well*-formed by the usual standards of the language, and yet for one reason or another make sense in the context in which they are used. For this latter type, then, we could reserve the term grammaticality. Thus, it is not well-formed to say "you no more fit drive can than Godsake", but it is grammatical in the dialect and/or register in which it is uttered---that is, among people who know the joke, such an utterance is not considered ill-formed in any way---on the contrary it conforms precisely to the rules of the dialect.

But after all, you may say, this joke is an isolated example of a phenomenon which rarely occurs in English or any other language, and thus it is hardly necessary for us to concern ourselves with the theoretical implications of it. This would be quite true if it were not for the fact that quite often there is some doubt as to the grammaticality of innovative utterances of native speakers of a language. They may be well-formed by all the rules we are able to state and yet strike us as awkward, unusual, semantically aberrant, or in some vague way undesirable. As evidence, I offer a few examples of actual remarks of native speakers of English

culled during the course of the First Annual LACUS Conference:

Question: "How much time do I have?"
Answer: "Well...we started late."
"It influences us on the subconscious level even when we are not aware of it." (the "even when we are not aware of it" was immediately labeled by the speaker himself as "redundant" and was deleted)
"What does Ross...er, not Ross...it's hard to distinguish certain figures on the horizon."
"This would be the *beth* methodology to follow."

Or let us take some examples which are a little more clearly oddly formed, if not ill-formed: *Breakfast will be eaten by Jim; Tennis is played by Tom;* or Peter Maher's example of the adjectival use of *fun* as in *That's a fun game,* or *Johnny's birthday party was funner than Bill's;* or how about one which I have heard from many people who ought to know better (including myself on occasion, I might add): *That's a whole nother story*". What is the past tense of *swing? Today I swing---yesterday I swang/swung/swinged???* Or *Today I dive---yesterday I dived/dove??* Or to take a phonological example, how about the omission of word final /t/ and /d/ in certain consonant clusters, so that we hear such things as *use cars* and *shave ice*; or the omission of the final /v/ of *five* in the Chicago pronunciation of a phrase such as *five dollars*. How are we to consider examples such as the above? (And we could go on endlessly listing such things.) Are they grammatical, ungrammatical, quasi-grammatical, pseudo-grammatical?

It is my contention that grammaticality is not always a clear-cut issue, and that in fact, between the black of ungrammaticality and the white of grammaticality there lies a vast gray area of indeterminacy where it is not at all clear, even in a single dialect, whether a given construction is grammatical or not. In another context Charles F. Hockett has said, with reference to morphemes, "If we were to class the morphemes of a language according to the degree of freedom with which they enter into combinations, we would find a virtually continuous scale of degrees of freedom, and unique morphemes would simply be those at one end of the scale".[2] I would venture to say that this could be regarded as equally true of the concept of grammaticality---ungrammaticality is merely one end of a continuous scale.

If we are willing to accept this possibility, it behooves us, as linguists, to examine how, if at all, we can deal with such a sliding scale. I do not intend to try to solve this problem here, but it is a subject which definitely bears serious investigation. In this paper, however, I would like to confine myself to pointing out the implications of such a sliding-scale view of grammaticality for certain approaches to linguistics, and particularly the transformational-generative school of thought.

As we all know, a constantly recurring theme of linguistic discussions among transformational linguists is the question of whether or not a particular item is grammatical. Countless hours are wasted arguing about this or that point of grammar without anything ever being resolved. The ultimate decision is always the same---my grammar is different from your grammar---it is grammatical in my dialect but not in yours. These discussions are basically fruitless and nothing is ever solved. Actually this is not quite true because I have it on reliable authority that James McCawley, at least, has hit upon a neat and fool-proof way of deciding on the grammaticality of any utterance. His solution is to present the utterance to his class for a formal vote. Only those officially signed up for the course may take part in the voting---after all, he points out, you must be registered to vote. (It is not clear whether one must additionally be a native speaker of the language in order to vote.) Thus the fate of countless sentences has been sealed forever through the unfailing accuracy of the democratic process. Well, for generativists less resourceful than James McCawley the question still remains---in writing a transformational-generative grammar, should the rules allow the particular construction in question, or not? Two transformational grammarians each writing a complete grammar of the same language no doubt would come up with two quite different grammars. Which one is right? Or is neither right? Is there some idealized grammar in which the different performances of the two linguists can be resolved into a single competence? But what is that competence? Does it coincide with the performance of one or the other linguist, or does it reflect still a third way which is different from either of the other two? And who is to decide what rules that idealized competence grammar contains? On what facts is his decision of what to include to be based? Or is it to be based on facts at all?

An alternative solution is to say that there is more than one correct transformational grammar of a language---each one reflecting a different dialect, or even idiolect, of the language. This, of course, is exactly what structural linguists have been saying for years---that when it comes right down to it we cannot hope to describe accurately and completely any more than a single person's speech at one and the same time.

Why is it then that structural linguists have never worried unduly about dialectal differences whereas transformational grammarians are constantly preoccupied with the subject? The answer lies in two facts: First, structuralists admit that their grammars reflect only one dialect, and that, to describe another dialect, additional or different facts will have to be accounted for. Transformational linguists have never accepted this premise, and the reason they haven't derives from the fact that, whereas structural grammars admittedly and intentionally are based on linguistic performance (i.e. behavior), transformational grammars are supposed to be based on the "higher and superior" level of competence. And it has never been resolved how many competences a given language may have. Are there as many competences as there are performances? If so then the division into two levels appears superfluous. Is there only one, or a small number, of competences which are then translated into a potentially infinite number of performances? But then the question recurs---who can decide what the competence grammar is, and how exactly is it trans-

lated into the various performances? Thus, transformationalists worry about grammaticality in different dialects because, "fuzzy grammar" notwithstanding, each and every example of dialectal differences, each and every disagreement about grammaticality, presents them with a difficult problem---what the rule should be in the competence grammar, which, after all, is the only one worth writing.

The second reason that transformational grammarians worry about grammaticality differences, while structural grammarians do not, is due to the very nature of transformational grammar. A transformational grammar is by definition a set of rules which generate "all and only the sentences of a language". That is, if something is allowed as grammatical there must be rules to generate it, and if something is disallowed as ungrammatical, then the rules must not be allowed to generate it, and there may even have to be specific rules which are inserted to disallow such a construction from occurring. This would be all well and good if all speakers of a given language could agree on what is grammatical and what is not, but this is never the case. Thus the transformationalists are constantly presented with the necessity of deciding on the grammaticality of an utterance before they can write a rule about it.

This is a failing, it appears, of all rule-based grammars. In this respect transformational grammar is probably the closest of any linguistic theory to the old prescriptive Latinate grammars of English and other languages. These latter suffered from exactly the same failing (among others) as transformational grammar---that there always seemed to be exceptions to the rules. We see in transformational grammar a real reversion to the principles of prescriptive grammar---there has to be a rule for everything---there can be no allowance for borderline cases, creativity, innovations, new forms generated by analogy and the like. Likewise there is no possibility of dealing with language change. Although transformationalists have made much of their purported ability to reflect sound change in their phonological rules, the truth of the matter is almost exactly the reverse. Because of the necessity to have rules which will generate some *one* final result, whatever it may be, they allow no leeway at all for the creation of new forms, new sounds, new grammatical structures. These would immediately be disallowed and deleted by the rules.

However we may ultimately decide to define grammaticality, and however we handle the analysis of grammatical utterances, once we have decided what they are, one thing is clear---a rule-based grammar, and most particularly a generative rule-based grammar, is simply incapable of coping with a sliding scale of grammaticality. At each point of a rule-based grammar, a rule must be written to specify, in effect, whether or not a given construction is grammatical---it is an either/or decision---either the construction is grammatical or it is not. There is *no room* for that aforementioned vast gray area between the white of grammatical and the black of ungrammatical wherein things may be sometimes grammatical, sometimes not; where they may be considered grammatical by one person but not by another; or where the question is muddied by

the fact that the construction does not overtly violate any stateable grammatical rules and yet is judged to be awkward, unusual, or undesirable.

Meanwhile, back at the railroad station, the Chinese businessman is still trying to retrieve his lost suitcase. In whatever way we linguists dissect his angry outburst, and whatever we decide about its grammaticality, the fact remains that he got his message across. He communicated to the baggage master just exactly what he intended, and the fact that the phonology, morphology and syntax of English did their best to get in his way did not deter him one bit. Somewhere, somehow, in our theory of language we have to make room for the Chinese businessmen of this world. That's all I hope!

FOOTNOTES

1. Cf. especially Adam Makkai "Degrees of Nonsense, or Transformation, Stratification, and the Contextual Adjustability Principle" in *Papers from the Seventh Regional Meeting of the Chicago Linguistic Society,* (1971) pp. 479-491.

2. Charles F. Hockett *A Course in Modern Linguistics,* Macmillan, 1958, p. 127.

ON THE GENERATION AND MODIFICATION OF GRAMMARS

John W. Oller, Jr.

University of New Mexico

Let me begin by saying that I am interested in psychologically real grammars. In that connection I want to discuss three closely interrelated questions: first, what are the necessary and sufficient conditions for the construction and/or modification of real grammars; second, what are the salient properties of the constructed grammars; and third, how might such grammars be constructed and/or modified on the basis of the necessary and sufficient information that is made available to language learners.

One of the main theses of this paper is that perhaps linguistics would progress more rapidly as a field of study if it focused on questions like, "how are grammars with certain salient properties constructed by learners" rather than on questions like, "what sorts of static devices can be invented to explicitly characterize sentential or other linguistic structures." That is to say, I am arguing for what I believe to be a more abstract and theoretically more powerful approach to the fundamental problem of characterizing real linguistic competence. Also, a theory of grammar construction and modification is in my judgment more directly accountable to the facts of the language use/learning continuum.

I will cite three sources of empirical evidence for the views advocated here: psychological studies of the general phenomena of perceptual defense and perceptual vigilance; studies of first and second language acquisition; and studies of pragmatic language tests. On the basis of evidence from such sources, I will argue that real grammars are generated and modified on the basis of inductive inferences that relate linguistic events to contexts via pragmatic mappings.

Someone may argue that what I am speaking of is somewhat pre-theoretical and programmatic, but this does not disturb me. It seems likely to me that the pre-theoretic reasoning which must precede and undergird any theory is probably as important if not more important than the eventually more explicit theory itself. Moreover, I am inclined to believe that if the past fifteen to twenty years of linguistic theorizing could have been undergirded by more appropriate pre-theoretic considerations and assumptions, perhaps more psychologically viable grammatical conceptualizations would now dominate the field. The point that I want to make in regard to this discussion, is that I am proposing a way of looking at certain well-known facts of language use and learning and I am claiming that such an approach has a number of important advantages when it comes to grappling with the sometimes fickle and variable facts of natural language use and learning.

There can be little debate about the fact that the necessary and sufficient conditions for natural language acquisition include being human, being motivated to learn to use a language and being exposed to the language in contexts where it is used for communicative purposes. Through the observation of my own children's first language acquisition, my own efforts at second language learning and teaching, ways in which I have often had to modify my own grammatical expectancies in the normal course of my experience (that is, I have had to learn new jargons, vocabulary, intonational systems, accents, and ways of conceptualizing), I have become convinced that language acquisition is chiefly dependent on inferential processes that relate utterances to extra-linguistic

contexts. One may complain, "but these are only introspective anecdotes and are scarcely admissible data for a sound theory of grammar construction," to which my answer is that these are exactly the common sorts of introspective knowledge that an adequate theory of language acquisition must explain. The lack of surprise value associated with such facts has been commented on many times by cognitive psychologists dating back to William James and as recently as Chomsky (1972). The interesting question is whether or not statements concerning such introspections can be made explicit enough to be translated into crucial empirical tests that will either prove them wrong or rule out as many other plausible alternatives as possible. Notice I didn't say that empirical tests might prove them right. The kinds of proofs that can be provided for the correctness of theoretical premises or experimental hypotheses are often either trivial or based on other premises which cannot themselves be proved.

One of the alternative solutions to the problem of language acquisition which may be ruled out if the argument for inductive inferences is sustained is Chomsky's "rationalist" position concerning language acquisition. If as Putnam has argued and as I will claim here, there exist certain general principles of learning which are capable of explicitly accounting for natural language universals and for real grammars in general, then as Chomsky (1972) has noted, the theory of innate ideas does not just postpone consideration of the problem of language acquisition as Putnam claimed, but it constitutes an empirically false explanation of how language acquisiton occurs.

Evidences that the grammatical processes which underlie language use and learning are not unique to language date back to the early writings of Karl Lashley and have been reaffirmed by recent experiments reviewed by Krashen (1973). There is much evidence to suggest that the kinds of "serial ordering mechanisms", to use Lashley's term, that are characteristic of language structure are also common to neurophysiological integrations of many sorts including the higher cognitive processes that have been traditionally considered "non-linguistic" in nature. I am thinking of the sort of cognitive processes that it is now fashionable to attempt to model with so-called "conceptual dependency systems".

If the bastion of innate ideas should succumb to the onslaught of an increasingly appealing empiricism, then what must become of the linguistically motivated revolution in psychology and closely allied disciplines? My prediction is that it will increase in importance. The linguistic revolution in psychology will, I think, increase in importance precisely because linguistics more than any other field of study has achieved at least some limited success in characterizing the kinds of processes that apparently not only underlie speech and language but are likely to help unveil the mysteries of all sorts of highly integrated, planned, and organized behaviors and cognitions.

One of the remarkable global properties of real grammars that is well attested in the psychological literature and can be characterized explicitly in grammars that incorporate a real time element is what may be referred to as redundancy, or conceptual dependency, or cognitive expectancy, or perceptual bias, or response bias, etc. It is this global property of real grammars that I intend to call attention to by using the term <u>expectancy grammar</u>. And,

expectancy grammars are not just linguistic devices in the terms of traditional and current linguistic theories. I believe that the real grammars which are internalized and used and modified by learners are to a very great extent characterized by the fact that they cause the learner to have certain sorts of expectations. While I say that the property of expectancy is a general one I do not mean it is necessarily a vague one. For instance, speakers of English know that certain possible phonetic segments are never realized in English. Similarly that there are intricate sorts of restrictions and constraints on possible syllables and syllable sequences. It is this kind of information coded in an expectancy grammar which makes it difficult for a speaker of English to understand the statement: [hizəθɚˈɡɛɾksɚˈjin] spoken by a New Delhi born M.D. visiting the United States. (He said, "He's a thoracic surgeon.") When he later added, [maiwaifsəsɚˈtɑləˈjɪst] I was sure at first that he had said she was a "psychologist", but I learned later that she was in fact a "cytologist."

Many sorts of grammatical expectancies can be characterized in the sort of grammar that Woods (1970, 1972) refers to as an augmented recursive transitional network model. For example, when you hear "the" at the beginning of a sequence of elements, if you are a native speaker of English you will probably assume that you are at the beginning of a declarative sentence; you will know that you are in a noun-phrase, you will expect a noun to follow and you are prepared to accept intervening adjectives, and so forth. When functioning as a speaker you say something like, "the man will be here shortly" you probably are assuming that your listener knows which member of the fuzzy set of possible men is the one that you are referring to. As Chafe (1974) has argued at length many subtle aspects of relative intonational prominences assigned to linguistic elements must be understood in terms of what the speaker assumes to be in the "consciousness" (or short term memory or attention) of the listener at the time of speaking.

In their LSA paper this summer, Bruce and Schmidt (1974) showed that the interpretation of simple stories about color books, cookies, and suppertime can only be adequately characterized in terms of a grammatical system (or conceptual dependency system) on the basis of which the plans, intentions, and belief systems of the persons in the story are inferred by the understander. That is, when Billie says he wants a cookie, you believe that he plans to eat it. In another example, from the paper by Schank (1974), when Joe, while mowing the lawn saw red on his toe, he believed that he had cut himself; as it turns out in the contrived example, he had stepped in some ketchup. The inferences in such cases are based on the sequence of events that one expects on the basis of an internalized conceptual system or expectancy grammar. A parallel case at the level of sentence structure from Woods (1972) is,

The boy was bucked off by the barn.

which is slightly surprising inasmuch as we expect an agent to be mentioned in the by-phrase. When a locative pops up we do a kind of mental double-take that is not unlike the re-routing that Joe does when he discovers the ketchup instead of the blood he expected.

As Schank mentioned, in many cases, the empirical consequences of conceptual dependency constraints, or grammatical expectancies, are demonstrable in

psychological experiments. The vast literature on perceptual defense and perceptual vigilance contains many demonstrations of the effects of grammar based expectancies (see Erdelyi, 1974, for a review). Briefly summarized from a linguist's point of view, perceptual defense and perceptual vigilance are cases where the syntagmatic and/or paradigmatic processing of sequences of elements is either inhibited (as in perceptual defense) or facilitated (as in perceptual vigilance) by grammar based expectancies and/or the concomitant attitudes associated with them. Roughly speaking correct expectancies and/or positive affect facilitiate processing while negative affect and/or incorrect expectancies inhibit processing.

Interactions between affective and cognitive domains are poorly understood, but there is some evidence from studies of pathological communication patterns that sufficiently negative affect (as in a power struggle) may put a complete stop to effective cognitive processing. An extreme case illustrating this last speculation is schizophrenia, perhaps a slightly less extreme example on the political scene is the argument over who's responsible for the arms race (for elaboration of the effects of human relationship struggles see Watzlawick, Beavin and Jackson, 1967).

In the case of cognitive processing, there are innumerable psychological experiments of various sorts which indicate that correct grammatical expectancies facilitate processing. An exemplary and oft cited type of study is the tachistoscopic demonstration that threshold levels for words in context are lower than for the same words in isolation. A more subtle demonstration of the importance of grammatical expectancies at the level of belief systems is the study by Manis and Dawes (1956) showing that attitudes toward controversial issues affect subject's capacities to fill in words deleted from passages of prose stating strong views on those issues. A more recent sociolinguistic experiment by Schaier, Cooper, and Fishman (1974) reveals that for Muslim Arabs who speak both Hebrew and Arabic, communication on scientific topics is probably more persuasive in Hebrew while on religious topics it is more effective in Arabic.

Someone may ask "but all of this is chiefly about perception, what about the productive side of language use." In his review of the literature on perceptual defense and vigilance, Erdelyi (1974) argues that distinguishing between "perceptual bias" and "response bias" is probably more a problem of language usage than of psycholinguistics. That is, a disposition to respond in a certain way to a given stimulus situation is probably not fundamentally different from an expectancy for certain kinds of elements to follow in a given context. Skepticism among psychologists concerning the distinction between response and perceptual biases is matched by an increasing skepticism among psycholinguists concerning a possible grammar based distinction between the comprehension and production of speech. Clark, Hutchinson, and Van Buren (1974) have argued that there is probably one basic underlying grammatical system that governs both the perception and production of language. On somewhat different grounds, Chafe (1974) has claimed important similarities between the speech acts of both speakers and hearers.

On independent grounds, I have been making similar claims as a result of research under the general heading of error analysis or interlanguage studies

and also investigations of various sorts of pragmatic language tests (Oller, 1974). Children learning a first language frequently make the same sorts of errors in imitating utterances that they make when producing utterances spontaneously (Cazden, 1965, and Slobin and Welsh, 1957). The same sorts of relationships between production and comprehension have also been observed in children learning a second language (Swain, Dumas, and Maiman, 1974). Moreover, second language learners at many stages of development and from a great variety of background languages tend to make many of the same errors in learning English as a second language (Richards, 1974; Ervin-Tripp, 1974; Dulay and Burt, 1974). From language testing research, remarkably high correlations between scores on tasks as different as taking dictation, filling in the blanks in a cloze test, essay writing, oral interview, various forms of paraphrase recognition, and various completion tasks indicate a surprisingly high degree of variance overlap (Oller, 1974). Also, error types across the different tasks tend to be similar in fundamental ways. For instance, if a learner has trouble in handling certain kinds of cross clause dependencies in taking dictation, he is likely to exhibit the same sorts of problems in doing a fill-in-the-blank cloze test.

If a person is understanding what someone else is saying, he is always to a certain extent anticipating what the person will say next. Granted he rarely knows exactly what will come next in any given context, but because of his grammatical expectations at any given moment he will be less surprised by certain elements than by others. Indeed, many times when listening to another person speak, one is capable of supplying missing elements that are either identical with or equivalent to what the speaker was about to say. Even in cases where the speaker says something which he did not intend, the listener is frequently able to correct the error and proceed even though the speaker may be unaware that he has committed a faux pas.

The producer of language from his own vantage point, on the other hand, is constantly anticipating what he will say next on the basis of what he intends to say, and he is also anticipating the likely effects of what he is saying, and what he will say, on his listener (Chafe, 1974). In an important sense, then, both the speaker and listener operate on the basis of an expectancy grammar.

If the foregoing reasoning is correct in any fundamental way, then the processes of language use and learning must be very closely linked. As Erdelyi (1974) puts it, "long term memory must continuously interact through executive control processes [or in our terms through grammar based expectancies] with on-going information processing" (p. 2). I would like to suggest that the same sorts of inferential processes that are available for the interpretation and production of utterances in normal communication are also the basis for the construction and modification of expectancy grammars. I believe that the main process whereby grammatical categories are initially hypothesized and later modified and refined (or "tuned up" as someone said yesterday) is inductive classification. If this argument is correct, and if expectancy grammars are <u>general</u> cognitive control programs (rather than language devices <u>per se</u>), we should hypothesize a stage at which an infant does not recognize repetitions of the same phonetic forms on different occasions and is unable to integrate the various experiences with those repeated forms in different

contexts. Similarly we should predict a stage at which a child does not recognize the same object in different contexts or on different occasions (Oller, 1969). Otherwise, if the infant's integrative perceptions of such entities were innate rather than learned, there would be little basis for claiming that classes of abstract objects are constructed by inductive inference rather than being innate.

Experiments reported by Bower (1971) have to my satisfaction demonstrated that infants before the age of about 20 weeks in fact do not recognize the same object on different occasions, or even in different though contiguous contexts. After about 20 weeks of age infants so recognize identities and are surprised by multiple simultaneous appearances of a familiar object in different locations.

Bower was actually interested in questions concerning the integration of various sorts of sensory inputs. He demonstrated that by about the age of two weeks children expected to be able to touch a seen object. Nevertheless, not until about 20 weeks did they associate different appearances of their mother as being representations of a single object or a unit set. I think this prediction concerning the young infant's perceptions of objects carries us well beyond trivial and obvious facts that we might say we knew apriori on the basis of common sense. Nevertheless, more subtle predictions can be made.

As Lois Bloom (1974) notes in a recent review, "it may well be that certain structures are learned after others because of the more complex perceptual cognitive distinctions which they represent, rather than their greater linguistic complexity" (p. 404). However, when complexity is defined in terms of the pragmatic mappings of utterances onto extralinguistic contexts, "linguistic" complexity, and fairly subtle hypotheses concerning the sequence of acquisition of grammatical categories can be defended. For instance, consider the fact that pragmatic mappings of simple noun phrases involving size specifying adjectives like "big," "little," or "large," "small," are more complex than pragmatic mappings of otherwise similar noun phrases employing color words like "red," "green," "yellow," etc. While the NPs of the latter type map onto simple pairs of object plus color-attribute, the mapping of object plus size-attribute entails a comparison either against a normative notion for the sort of object in question or it entails a comparison between the indicated object and others present. If acquisition of these lexical items and their corresponding rules of usage is inductively based on such pragmatic mappings, young children learning English should be able to respond appropriately to directives employing color attributes before being able to respond appropriately to directives involving size attributes. To illustrate, "Bring me the _____ _____" where a color word is inserted in the first blank and where an object label is inserted in the second and where several blocks, balls, or trucks, etc. of different colors are available in the extralinguistic context is a simpler instruction pragmatically than "Bring me the _____ _____" where a size word is inserted in the first blank and a noun like block, ball or truck is inserted in the second and where large and small blocks, balls, and trucks are present in the extralinguistic context. As far as I know, no other theory of language acquisition is apt to propose a sequence for the acquisition of size and color adjectives, and on the basis of an autonomous syntactic theory there is certainly no argument for predicting such an order. There is no reason that a

theory of innate ideas should predict that phrases like "big truck" should appear later than phrases like "red truck."

While I have not gone to the trouble of checking this prediction on many children, I did check it on three children at about two years of age, all of whom were able to respond appropriately to NPs including color words but none of whom were able to respond appropriately to NPs including size words. They would invariably bring one of two or more objects available, but they were as apt to bring the "little" one as a "big" one. Since I was intrigued by the result with my own two year old daughter at the time that I tried the experiment; I set out to teach her how to pragmatically map NPs involving size words. I very carefully arranged sets of toys of different sizes but the same color within each set. Then I set out to show her the difference between "big ones" and "little ones". I ran into some trouble, however, when I picked up the bigger of two yellow blocks and told her that the toy was a big one and she corrected "no, Daddy, yellow block." I tried expanding, "right, it is a big yellow block." "No, Daddy, yellow block"; after I got the same response on a couple of other tries I had to give up because she began to cry.

Recently, in reading over the dissertation of Cazden (1965) I was reminded of that event by Cazden's finding that modeling of utterances was a more effective tutoring procedure than expansions of the child's own productions. In fact, although modeling helped very little, expanding the child's own utterances helped not at all. What this may show is that at any given stage, the child's grammatical development may be fairly rigidly determined by inherent limitations on the child's short term memory governed by the sorts of pragmatic mappings that the child has learned to handle to that point. Adult expansions of a child's utterances would practically always exceed the child's concurrent level of grammatical development. Modeling in pragmatically rich contexts as in the case of Cazden's tutors would on the other hand more frequently provide just the sort of information that could cue the next appropriate grammatical advance.

Of course, there have been many other studies of sequences of grammatical category acquisition that should be examined in this regard. Up-to-date references are provided by Dulay and Burt (1974) and by Ervin-Tripp (1974) concerning parallels between sequences of grammatical category acquisition in first language learners and in child second language learners.

No doubt that most interesting longitudinal and cross sectional studies have yet to be designed, but a word of caution is in order as Bloom, (1974) admonishes, "it is unreasonable to consider language development apart from the concomitant cognitive development to which it is directly related; i.e. one cannot explain the words and linguistic structures that the child learns unless one understands something about the child's experience of events in the world, . . ." (p. 410) and again later on "what children do as they learn to talk--how they respond to the speech that they hear, and how they use progressively different words and structures in their interactions with the world --must provide the only evidence for an explanation of language development. And any theory whether linguistic, psychological, or psycholinguistic, will be explanatory only to the extent that it can account for the relevant data" (p. 412).

In the terms I have outlined here, the relevant data are the learner's own pragmatic mappings of utterances in the contexts of communication, and the progressive development of those mappings in the direction of those performed by mature speakers. I have argued that the basic problem of linguistics is to explain that development and I have suggested that it may in fact be based on the same sorts of inferential processes whereby novel utterances are produced and/or interpreted.

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Some Observations on Jespersen's Analytic Syntax

Ladislav Zgusta
University of Illinois at Urbana-Champaign

Jespersen's is one of the most detailed and probably the most important English grammar in existence (Jespersen 1913 ff.); his Analytic Syntax (Jespersen 1937) is a book in which an author with a lifetime of experience shows himself not only a man of erudition but also a bold methodological innovator: is this not enough to invite interest and study?

In a recent article of mine[1] I gave a general appraisal of Jespersen's method; I mainly tried to discern when does Jespersen focus his attention on the grammatical surface and when does he penetrate deeper. His general policy is to stick as closely as possible to the words as they appear on the surface. He is not much concerned with their inflectional forms, with morphology; for instance, it can be shown that Jespersen discerns by his analytical formula the following two Russian sentences: On byl soldat "He was a soldier" S V P, and On byl togda soldatom S V 3 P "He was a soldier at the time." But a Russian sentence equivalent in sense to the second sentence gets the same analytical formula as the first, to which it is equivalent in the constituent words: On byl soldatom S V P "He was a soldier at the time" (9.1)[2], because the difference is not expressed by a word (as togda), but by a case of the predicative word. Latin expressions like licet mihi esse otioso or quisque suos patimur manes are too complicated by their rich morphology for Jespersen's notation, which certainly was based primarily on the structure of English.

When trying to understand Jespersen or even when appraising him, it must always be remembered that he frequently accepted the possibility of analyzing one expression in more than one way. Fo for instance, Lat. vivit, It. vive: "V or S^o V" are Jespersen's two analyses (7.1), one as a mere verb, the other as a verb with a latent subject. Sometimes analyses vary from passage to passage, e.g. It. canta understood as S^o V, a verb with a latent subject (2.3), but Lat. amo puellam understood as {S V} O, i.e. a verb with an inherent subject, plus object (2.4). Such variation is to be understood as a part of Jespersen's approach, which frequently allowed more than one way of understanding an expression. We shall see that this is an important feature of Jespersen's method. To show how closely Jespersen is able to stick to words, let us quote (8.3): Fr. il s'en souvient S O (S) O_2 V. The symbol **O** (S), that is "object implemented by the subject" refers to the reflexive pronoun se. Everybody will object that se is no real object (only en, represented by O_2 is one); the verb se souvenir is reflexive only in its surface. Jespersen knows this and therefore analyzed (8.5) il se sert du couteau as "S W O or S O (S) V p O_2": that is, he either took se sert as a complex verb (W), or se as O (S), as in the previous case. The latter notation is inspired by Jespersen's concern with words as they appear on the surface. This concern with words (and morphemes) occasionally leads to an interpretation which is dictated by etymology; cf. me voici O/S V 3 (8.8): that is, me is considered either as an object or as the subject (see below about this) and voici as a form that unites a verb and an adverbial. If we compare this with eccolo 3 O/3 [3] (8.8) we see that the difference probably is caused by the proximity of Fr. voir. In any case, a varying treatment of verbs, not only the reflexive and quasi-reflexive ones, is frequent; cf., e.g., He was in love with her "S V P (p 1) 3 (p 1) or S W O" (9.3). W represents here the possibility

of taking to be in love with as a complex verb, whereas the first formula takes the words separately: in love is a predicative, with her an adverbial, both expressed by a preposition followed by a word of first rank. Here we begin to see that Jespersen's notational variation sometimes implies a variation of depth of analysis. This is not always the case. Cf. He lived as a saint S V p P but Il a agi en homme conscientieux S* V p 1 (P* (1 2)) (so in 9.5): a predicative necessarily belongs to the subject and so the asterisks (the defined purpose of which, in Jespersen's notation, is to show what belongs together, if the connection is not clear by itself) are as unnecessary in the second analysis as they would be in the first one.[4] Vacillations of this type may be irritating. Cf. (9.4): natura humana aevi brevis est S (1 2) P (1 2)[2] V; der Mann war guten Mutes S V P (2 1)[2]; but animo meliore sunt gladiatores P (1 2) V S. All three sentences consist of a subject, verb, and predicative; all the predicatives are said to consist of a first-rank substantive and a second-rank adjective (in different order) - but why are only the first two predicatives said to be converted into a second-rank adjective (see the supralineal[2]), when the difference between the genitivus and the ablativus qualitatis would seem to be irrelevant? Sometimes Jespersen is able to offer three alternative analyses: cf. He looked (stared) me in the face: S V O p 1 (subject, verb, object, preposition, first-rank noun), or S W (complex verb, comprising everything with the exception of he), or S V* O p 1*, with asterisks showing the relation (36.7); obviously, Jespersen felt that at a deeper level, the activity expressed by the verb is more directed at face than at the formal object me.

No need to say that one does not always agree with Jespersen in his analyses.[5] So for instance: Germ. Es fährt sich gut auf diesem Wege S V O (S) 3 p 1 (2 1) (so in 8.3), because sich cannot be the object of the verb, even if it is taken to be identical with the subject. We are here in a field which we might call phraseological, or idiomatic syntax: some syntactic patterns do have (in combination with a suitable lexical insertion and/or context) a meaning of their own. Thus Germ. Er kämmt, wäscht, etc. sich; sie kämmt, wäscht sich; but es fährt, liest, trinkt, etc. sich gut, schlecht etc.: "He, she combs, washes, etc. himself, herself", but impersonally "Driving, reading, drinking, etc. is good, bad, etc." No wonder a situation of this type creates difficulty: phraseology, or idiomaticity always creates some deviation from the normal patterns. We have seen above that Jespersen had some trouble with phraseological verbs (W), no wonder that this "phraseological syntax", much more elusive than phraseological lexical items, caused difficulty as well.

However, there are many cases in which Jespersen gives interpretations of admirable subtlety just because he ventures to go beyond the immediate surface of the sequence of words. A few examples. What a pity that he should die so soon! P (2! 1) S (3^c S_2 V 3) (so in 9.6), that is: the clause introduced by the third-rank conjunction that is a subject, to which the first clause, introduced by the second-rank exclamatory word what is a predicative. Jespersen is here some decennia before his time. Similarly: He was in a mood to cry S V P (p 1 p I) (so in 18.2). The prepositional phrase is taken as a predicative, possibly on the analogy of an expression like sulky, certainly more because of the deeper sense than because of the surface.[6] Another example (8.6): All

men are not born to reign S (2^{q*} 1) V 3^{n*} P (Y p I). The analysis shows that the negation not belongs to the quantifier all in the subject[7]; this opens then the possibility to list and to analyze all the six readings of the sentence John told Robert's son that he must help him.

Again, we can see that the depth of the analyses is not always identical. Let us compare on the one hand (16.1): She was made to cry 1/2 S V^b 1/2 S (I). This means that the infinitive to cry[8] is part of the subject, the other part of which is she. If this analysis goes very deep, indeed, so deep that not everybody will be ready to accept it[9], it has the advantage that it tries to show that the causation (was made) pertains both to the person (she) and to the effect (to cry); that we try to discern these two in traditional syntax as persona affecta and effectus causae is another thing. Jespersen goes here his own way. Compare with this, on the other hand (2^4.9): He may have heard us, but if so he did not show it S V O & 3 (3^c 3) S V^n O_2, where Jespersen does not care to indicate the coreferentiality of so and it, nor their reference to the first clause, say S V*,** O* & 3 (3^c 3**) S V^n O_2**.[10] In my previous article, I took the position that such differences in the depth of interpretation are not the result of Jespersen's not seeing the possibility of a deeper interpretation, but of his not wishing to express it. To the examples discussed there, one could add, e.g., the following (18.3): The story is too long to be read at one sitting S V P (3 2 p 1 (I^b 3))[11]; the story is too long to read at one sitting S* V P (3 2 p 1 (I O^{o*} 3)): the second analysis indicates the coreferentiality of the subject (the story) and of the latent object of the active infinitive. Surely there can be no question that Jespersen perceived the conceptual identity of the two sentences; however, he was more interested in capturing their closer-to-surface difference.

Another type of vacillation can be illustrated by the following examples: I consider this a lie S V O (S_2 P) (so in 2.1); He amuses himself S V O (S) (so in 2.4). The difference is in the value of the parentheses: in the second sentence, the parentheses give a semantic interpretation that the object is identical with the subject and the formula could be rewritten as S* V O*; in the first sentence, the object really consists of a phrase which can be taken as a(nother) subject and a predicative (this [is] a lie). Cf. (27.1): The...oracles...have informed us...that the waste of our fortune... S V O O (3^c X p O...[12]. Notice that the second O stands here for the whole following clause, as manifested by the words symbolized to the right of the parenthesis. Compare with this (2^4.4): Es kommt darauf an, ob er bezahlen wird S V* 3 (1**-p) 3* 1** (3^c S_2 V). An idiomatic translation would be: "What matters is whether he will pay". Jespersen's notation indicates that ankommen is a verb split on the surface (V*...3*). More importantly, his notation indicates (by the means of 1**...1**) that the whole clause (ob er bezahlen wird 3^c S_2 V) belongs to the deictic dar(auf) of the first clause. Surely Jespersen had at least two possibilities: either to take the second clause as the subject of the first (Whether he pays: this matters), or, judging by other parallels, as an object of the thought expressed in the first clause and referred there to by the deictic dar(auf). He chose neither possibility and wrote 1: a noncommitting symbol "element of the first rank", which he frequently uses for substantives.

This symbol 1 (probably one of the weakest points in Jespersen's

analytical and notational system) is used in the same noncommitting way in other analyses as well. Cf. (10.3): I found John in the garden S V O p 1 (=3 (p 1)): here he indicates first the merely morphological analysis p 1, but then explains that these two together function as a third-rank adverbial. Thus, we get the two analyses, one merely morphological, the other syntactically functional. However, in (23.4): By the time you are dressed, breakfast will be ready p 1 (1 2 (3^c S V P)) S_2 V P we do not get any statement on the syntactic value of the first clause (which would, of course be 3 (p 1 (etc..., since it functions as a third-rank adverbial. If we go back to the German sentence, we can easily see that Jespersen's second 1** is a mere dummy, there being no substantive which it would represent; indeed, its only value is in avoiding the choice of either S or O and thus of indicating the syntactic function of the second clause.

On the other hand, there are cases where Jespersen did not hesitate to indicate that there are various levels of interpretation equally valid at the same time. E.g. (17.7): I find this impossible to believe S V O (S_2 (O*) P p I*). This means: all that follows after this (incl.) is the object of I find; at the same time, this is the subject of the clause and the object of believe. I think that analyses like this belong to the best achievements of Jespersen's method. Let us, however, mention another thing: no doubt that the predicative is the whole phrase impossible to believe; therefore, the notation should be P (2 p I). But this is exactly where Jespersen does not wish to be: we would like him to analyze the sentence into purely syntactic elements first and then indicate by which morphological categories, or if it please you, words, they are manifested. This is what Jespersen does not do; at least not always. Cf., on the contrary, an example where he does it (24.5): I'll answer next time I see you "S V 3 (2 1 2 (3^{co} S V O)) or 3 ((2 1)^c S V O." This means: next time etc. is a third-rank adverbial element to I'll answer. The adverbial clause can be understood in two ways: either the first-rank element time is preceded by the second-rank next and followed by another second-rank element, which consists of the clause itself (S V O) preceded by a latent conjunction (notice that the second 2 is a dummy symbol again); or, next time is the conjunction itself (2 1)^c. Obviously, Jespersen is fully capable of going beyond the surface to the syntactic function if he chooses to do so. Another example (10.1): I feel a great deal better S V P (3 (2 1)^q 2). This means: a great deal (2 1) is a quantifier (cf. much) that serves, as a whole, as a third-rank modifier of better; and the whole phrase is not an adverbial (third-rank) element, but a predicative in the sentence: surely an interpretation that goes beyond the surface.

It is interesting to notice that Jespersen frequently deals with lexical semantics in a highly penetrating way. I do not mean so much simple cases as (6.5) a cold December night 2 1 (2-1) but an early December night 2 (2-1)-1 or 2 (2^m 1)-1 (because the letter means "in early December"), or new and second-hand bookseller 2 (2 & 2 (2 + 1) 1)-1 ("new books, second-hand books"). Analyses like an utter fool, a perfect stranger, an awful swell, a comparative rarity, absolute necessaries, all of them 2 (3) 1 (so in 3.6) are more interesting, though one would like to hear how the class is constituted: e.g., is total fallacy, absolute omnipotence 2 1 or 2 (3) 1? Very subtle is the analysis of the

implied predicatives (3.7): a born poet 2* 1 (P*); this means: the first-rank noun poet is modified by the second-rank born, which, however, goes with the predicative function of the noun, cf. born as a poet. In the same way a dismissed director, a designed bishop, a prospective bride, eine geborene Rantzau 2* 1 (P*), le malade imaginaire 1 (P*) 2* (qui se croit malade); a seeming widow and a secret bride 2* (Y) 1 (P*) & 2* 1 (P*). Somewhat different (and more problematic) is (3.8): in my young days where we get two analyses: "$(1\ 2)^2$ 1, if we do not prefer $1^2\ 2^m\ 1$." The second analysis sticks to the surface: the first-rank days is preceded by a second-rank modifier (young) and by a first-rank element converted into a second-rank one (I→my); the first analysis goes beyond that: young belongs to I (inherent in my). A similar depth as here is attained in a would-be wit 2 (Y I*) 1 (P*) (so in 21.4): the first-rank wit is modified by a second-rank element consisting of a participle (Y) and an infinitive, to which wit is predicative: somebody who would like to be considered a wit.[13] And again, besides this deep interpretation we get a close-to-the-surface one: Y^w instead of (YI*), i.e. simply "a complex participle." And a final example: Jespersen perceives (8.4) that the boys fight, we meet have the formula S V O^o (S_x); that is that the latent object is the reciprocal subject. If we compare with these analyses those cases where Jespersen simply chose to treat a complicated verbal phrase as W "a complex verb", we cannot fail to see that the depth of his analysis depended on his decision how deep to go.

There are other cases where a certain degree of uncertainty, or what would seem to be vacillation, is not caused by Jespersen's decision, but by both the data and Jespersen's approach. Let us start with his treatment of apposition. First, some cases that would seem not to allow any doubt: Brown, never at a loss for an answer, replied S [3^n p 1 p O] V (so in 4.3). The brackets indicate the apposition (with the fine identification of answer as its object). 4.5: c'est une impertinence que cette histoire "S* V P [3 2 1]* or s V P S (3 2 1)." This means: in the first analysis, impertinence is predicative to c'est; follows the bracketed apposition that belongs to (or explains) the subject ce.[14] In the second analysis, the parentheses show what belongs to the subject of the sentence S, which is anticipated by the minor subject s (ce) (cette histoire est une impertinence). Both analyses are equally possible and ingenious, the second going somewhat deeper than the first one. However, there are examples where we do not agree (4.2): She presided with a grace all her own S V p 1 [3 S^2 2]: how could all her own be an apposition to grace?[15] Certainly S V p 1 3 S^2 (if not S V 3 (p 1 3 S^2 2)) would seem to be closer. Or (both 4.2): I like my tea very hot S V O (S^2 1) [32], Ich trinke den Kaffee gern warm S V O 3 [2]: how can very hot, warm be appositions? Surely both are predicatives, say S V O* (S^2 1) P* (3 2) and S V* O** 3* P**, respectively. Jespersen must have felt that his analyses were not accurate enough; therefore, he has (22.5) Whom the gods love die young S* (O^c S_2 V) V [2*], where he still classifies young as an apposition, but shows by the asterisk its relation to the subject - surely a predicative again, S (O^c S_2 V) V P. Fundamentally different is the difficulty with Germ. Er kam gelaufen S V [$Y^{b/a}$] (so in 21.3).[16] This would seem to be another phenomenon of what we called "idiomatic syntax"

above: the two verbs semantically combine to describe how somebody arrived: "He came running." The phenomenon is well known in Greek, where it has stronger effects, e.g. éphthase klépsas "he overtook having stolen = he was the first (before others) to steal"; láthe biósas "remain hidden having lived! = do not live ostentatiously!" etc. To treat a phenomenon like this as apposition is to put the phenomenon into a category so broad and vague that it has no explanatory power. It would seem to me that most sentences listed and analyzed in para. 16.2 are semantically very close, but Jespersen treats them as cases of split subject: She happened to notice it 1/2 S V 1/2 S (I o), that is, both she and to notice it are treated as the subject of happened. Similarly She is sure to notice it 1/2 S V P 1/2 S (I O), She seems to notice it 1/2 S V 1/2 S (I O).

Cf. yet another application of the so-called apposition (22.3): He had no idea how this should be done S V O (2^q 1) [3^c? S_2 V^b], where the dependent question is treated as an apposition to the object. Proceeding yet further on this dimension, we find not only what Jespersen calls ordinary parenthetic remarks symbolized as appositions or extrapositions, e.g. This, I think (or it seems), is madness S [S_2 V] V P (so in 25.1) but also what Jespersen calls the speaker's aside with a notation of its own (25.9). So e.g. Talking of golf, have you met Nelson lately? <Y p 1> S V O 3?.[17] Oddly enough, I met him yesterday <3 4> S V O 3. The latter example shows that there can be an overlapping with sentential adverbs; but Jespersen uses this notion even more frequently for evaluative adverbs: He is a regular magician S V P (<2> 1); He fell rather than climbed into bed S V <3 3^c V> p 1: surely a practice that must result in many overlappings and borderline cases. However, this is one of the not numerous cases where Jespersen seems to be led primarily by semantic considerations to establish a separate class.

I mentioned in the previous article on Jespersen (V. Zgusta, op. cit.) that Jespersen has the following analyses of impersonal verbs: Germ. mich friert, mir graut O/S V; this means that he takes mich as the object, which it morphologically undoubtedly is, but at the same time as the subject, possibly on the analogy of ich friere. I shall not repeat here what I stated about the various possibilities of interpreting such verbs. Let us, however, mention other aspects of the problems: What ails Tom? S/O? V O/S (so in 8.8). This is fully in line with the other examples: Pudet eum sceleris V O/3 3/O, Mich reut dieser Tat O/S V 3/O (2 1) indicate the possibility of taking the object as a tertiary (an adverbial element) as well. With this we may compare She nodded her head S V O/3 (S^2 1)[18]; He struck her a heavy blow S V O O_2/3 (2 1)[19]; It costs (us) five shillings S V (O) O/3 (2^q 1),[20] (all in 7.4).[21] Interpretations of this type go very deep. I am afraid that agreement may vary from reader to reader and from case to case. Myself, I am unable to understand sceleris or five shillings as "tertiaries"; a heavy blow could be understood as such: after all, we have here a case of the "accusative of content" of traditional grammar, which semantically complements the verb in many languages; and She nodded her head has at least two readings (one literal, the other as a sign of affirmation). However, it is not this possible disagreement which we wish to stress here. The important point seems to be that Jespersen does not attempt to make an either-or decision. I find this methodically very important. Even if categories, classes, patterns, etc.

are well established, there are borderline cases and it is better to treat and indicate them as such. More than that, language sometimes seems to function not by being as clear and explicit as possible, but by hinting in different directions at the same time. While most ambiguities are disambiguated by the context, Hockett's classical example[22] John, leaping onto a runaway horse, quickly brought it under control and his point that it is natural for language that the clause leaping...horse is understood both ways is valid. Of course, any grammarian of any school of thought can give either-or analyses of the sentence; but it really seems that the hearer takes the sentence globally, not with an absolute clarity as to where the clause belongs. Most of the apo koinoû constructions (as they are called in traditional grammar) operate on this principle.

In my previous paper on Jespersen, I said that in my opinion, Jespersen's way of deciding from case to case how deep he would go in his analyses or how far away from the surface, while not allowing him to construct a unified theory with a rigorous method, keeps him always reasonable and "natural." To this we may add that his allowing various interpretations also is a great advantage of his approach.

Footnotes:

1 "Quomodo superficiei grammaticae nexus sensusque profundior in Jespersenii syntaxi analytica dicta tractentur," Festschrift for Herbert Penzl, to appear. A bibliography of scholarship concerning Jespersen's Analytic Syntax is to be found in that paper; of particular importance is J. McCawley's review of the second impression of Analytic Syntax, Language 46, 1970, 442-449.

2 Jespersen's Analytic Syntax is quoted by its sections in parentheses.

3 A possibly more correct symbolization would be 3-0/3 (two morphemes united in one word), but it is not necessary to be too pedantic about it.

4 We do not wish to analyze other differences, so, e.g., why should the second sentence indicate that the predicative is a realization (consisting of a first-rank noun and second-rank adjective) of a first-rank noun (1), whereas the first analysis gives directly P. That both as and en are analyzed as p (preposition) is given by Jespersen's general approach.

5 We are talking here about single points and interpretations, that is, we take Jespersen's system as it is and may agree or disagree with its application in single interpretations. An overall criticism of the whole system would require a discussion of such problems as the mixing of syntactic and morphological categories (S, O etc., but 1). See (to some extent) Zgusta (V. footnote 1) and the bibliography indicated there, and some remarks below, in this article.

6 It may strike us as strange that to cry is taken as a preposition plus an infinitive. This is a part of Jespersen's system as a whole, which we do not discuss here.

7 The predicative is said to consist of a participle (Y), preposition (to), and infinitive.

8 Jespersen does not find it necessary to capture to as a separate item, in this case.

9 The parallel interpretation The door was painted red 1/2 S^r V 1/2 S^r (P)

(so in 16.1) would seem to be improbable in any case.

10 & stands for but, followed by a third-rank element (3) manifested by a third-rank conjunction (if) and a third-rank adverb (so); S and S are coreferential, O and O_2 not, by the rules of Jespersen's notation.

11 The P(redicative) consists of a third-rank adverb (too), second-rank adjective (long) p (to), and a first-rank element, in its turn consisting of a passive infinitive and a third-rank element (at one sitting), which is not further analyzed into, say, 3 2^q Y: Jespersen does not go into such details if they are out of the scope of the problem just discussed.

12 O is the indirect object, X what Jespersen calls nexus-substantive (like work, waste, invitation) that is syntactically close to a verb. p stands here for of, our fortune is expressed by O (O_2 would have been better, if not O_2 (1^2 1)).

13 Doubts might arise whether Jespersen was right in labeling would as Y, an infinite, participial form. He was: the Czech parallel expression is rádoby chytrák. The predicative is masculine, therefore by congruence the expression goes to rád by byl chytrák; in the same way rádoby krasavice "would-be beauty" goes to ráda by byla krasavice (feminine); in both cases, the neutral form rádoby is really used, undoubtedly as the most infinite, unmarked one.

14 The moot character of que as a third-rank element is irrelevant in this connection.

15 her is a conversion of she; therefore, S^2.

16 $\overline{Y^{b/a}}$ means a participle which is either passive or active. It would serve no purpose to discuss the label at length.

17 Small v is sometimes used to symbolize auxiliary verbs.

18 S^2: her converted from she (S).

19 Notice that her is here O, direct object; cf. the following sentence It costs (us) five shillings S V (O) O/3 (2^q 1), where us is O, i.e., indirect object. Jespersen probably felt that omitting her in the first sentence would change it.

20 Cf. Such books cost a lot to print S (O*) (2 1) V O p I* (so in 25.5): obviously, the surface is captured here, whereas para. 7.4 tries to "cut deeper" (cf. Jespersen page 56). A good example of how Jespersen sometimes went deeper, sometimes less so, out of his own decision.

21 Wir laufen schlittschuh S V O/3 (also 7.4) is an error: schlittschuh can be only an adverb.

22 Hockett, A Course in Modern Linguistics, New York 1952, 246 ff.

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THE CLASSIFICATION OF RELATIVE CLAUSES[1]

William Card and Virginia McDavid
Chicago State University

This study is concerned with the classification of relative clauses, often called adjective clauses, appearing in such constructions as "A book which I read," "A man whom John knows," "A remark that she made," and "The bread you made yesterday." The function of these clauses is to modify a preceding construction, most often a preceding noun phrase. We limit our discussion to five relative words: which, who, whom, that, and ∅ (zero). Who, whom, and which as interrogatives or in other uses and that in its other uses have been excluded.

The classification is derived from our study of random samples of clauses from 56 books of expository prose written by native Americans on a wide variety of topics and published between 1955 and 1965. Computational Analysis of Present-Day American English by Henry Kucera and W. Nelson Francis (Providence, Rhode Island: Brown University Press, 1967), based upon the Brown University Standard Corpus of Present-Day Edited American English, was not available to us in the earlier stages of this study. However, the materials we considered fit into categories D, G, and J in the Brown Corpus. The clauses were collected by our students, chiefly candidates for a master's degree, and checked by us.[2]

Our objective was to study the circumstances in which these words appeared as relative pronouns. What were their grammatical functions? Did a preposition ever appear at the end of a clause? What was the frequency of the various words? And to what extent was the advice of usage books that which should be used only nonrestrictively followed?

The number of words read to reach 100 consecutive clauses of this kind varied widely. The average was about 11,500. At one extreme was a low of 4000 in John Kennan's On Dealing with the Communist World. At the other extreme was the 19,000 words words read to reach 100 clauses in Richard Neustadt's Presidential Power.[3]

Turning to the set of tables labeled 1, we see from Table 1D that which is the most common of these relatives and whom the least common. And from Table IE we see that these words function most commonly as subject. They are almost equally common as direct object and object of a preposition and least common as complements. There were no examples of them as indirect objects.[4]

Looking at Table iA, we see that both which and whom are more common as object of a preposition than as direct object. In the examples where which is the object of the preposition, the preposition occurs at the clausevery rarely, and with whom as object, even more rarely.[5] Here is an instance in which the advice of usage books that a clause should not end with a preposition reflects actual practice.

With that and ∅, of course, the preposition must occur at the end of the clause ("Every man has an equal chance for the well-being that is essential to the enjoyment of the freedom that we brag about.") The feeling that

ending the clause with a preposition may be awkward or make for difficult reading may be responsible for the small number of times it is found there. And almost surely this restriction on the use of that and Ø is responsible for the high use of which as object of a preposition (28 percent).

The average number of occurrences of each of these words appears in Table 1G. There were wide variations among authors. In the selections from three authors, more than 70 percent of their clauses were which clauses, and four of them, among them Kennan, Senator Fulbright, and Samuel Morrison, have more than 60 percent which clauses. At the other extreme are three authors with fewer than ten percent which clauses. The topic of discourse makes some difference, but it is possible to find books on sociology at both extremes. And President John F. Kennedy's Profiles in Courage has 50 percent which clauses, his brother Robert F. Kennedy's The Enemy Within only 12 percent. Yet both books deal largely with persons.

A similar wide variation exists with occurrences of who, with extremes ranging from 60 percent to a low of 10 percent, both in books on sociology. In only a little over two percent of the clauses is whom the relative word. Eleven authors manage to avoid using whom at all, and 39 used it fewer than five times. Only one of them, Robert Kennedy, used it more than ten times. His book is one we would characterize as being informal in tone, yet complex syntactically.

There is a similar variation in the number of occurrences of that, from an extreme of 58 percent of the total number of clauses to a low of one percent. For the zero relative, we also find extremes of variation. Three authors use it not at all, and two use it for more than 25 percent of the total number of their clauses. The absence of a relative word might seem to make it difficult to determine the place in the sentence at which the relative clause begins. In practice this is not so: about 95 percent of these zero clauses have as the first word of the clause a personal pronoun, and many which do not begin with a personal pronoun have instead a proper noun.

Few grammars or usage books treat the grammatical functions of the zero relative, and those which do usually state that it cannot be used as subject. Yet one finds such examples as "He knows all the dodges [Ø] there are for padding accounts." And "Audience receptivity is the only valid receptivity [Ø] there is." The zero relative is also used as a complement, as in "He would live like the gentleman he had supposed himself to be."

The question of whether these words may be used to introduce both restrictive and nonrestrictive clauses is a common one in grammars and usage books. To summarize their advice briefly is difficult and leaves out subtleties, but the consensus is this: Who and whom may be either restrictive or nonrestrictive. That is preferably restrictive but nonrestrictive that occurs. Which is preferably used only in nonrestrictive clauses although, regretably, it occurs in restrictive ones.[6] Turning to Table 2D, we see that more than 80 percent of all clauses are restrictive and that all examples of that and Ø are restrictive. And

in Table 2A we see that nearly 73 percent of the occurrences of which are restrictive. Plainly the statements about the restrictive and nonrestrictive uses of that and which should be replaced by some statement like this: Thatis always restrictive. Which may be either restrictive or nonrestrictive, depending upon the author's taste and style.

Many questions about these clauses are untouched by this study--the nature of the antecedent of the pronoun and whether there are intervening elements between the antecedent and the clause. Further work with data should make it possible to establish some actual facts about usage and remove it from the realm of unsupported opinions.

1 Some of the data in this paper was presented by Mrs. McDavid at a meeting of the Midwest Modern Language Association in Cincinnati on 18 October 1968.

2 Within the limits stated our students chose their own books and, starting somewhere after the first chapter, marked and numbered 100 consecutive clauses--consecutive to guarantee the randomness of the sample by eliminating selection by the reader. One of us checked for errors and omissions. The entire sentence in which the clause occurred was then typed on a 4 x 6 slip. Each student classified his set of slips in ways we designated. We checked all classifications. To the students who produced our raw material we are most grateful.

3 A study of which clauses in the Brown Standard Corpus reveals another sort of difference in the frequency with which these clauses occur: they are twice as common in informative prose as in imaginative prose. See Virginia McDavid, "Which in Relative Clauses: Based on an Examination of the Brown Standard Corpus," in Festschrift for Thomas Pyles, Berry McIntire and Walter Meyers (Eds.), forthcoming.

4 No examples of which as indirect object were found in the materials from the Brown Standard Corpus used in the study above.

5 Again, the materials from the Brown Standard Corpus had very few clauses with the preposition at the end.

6 For a fuller statement of current statements about that and which in restrictive and nonrestrictive clauses, see the article mentioned above. The percents of restrictive and nonrestrictive clauses in the Brown Standard Corpus is almost exactly the same as found in the present study.

TABLE 1A

Percents of each word in its several grammatical functions

	Subj.	DO.	OPr.	Complement
which	59.4	12.3	28.2	0.1
who	100.	0.	--	--
whom	--	36.6	63.4	--
that	82.9	15.5	1.4	0.08
Ø	0.8	90.8	7.7	1.

TABLE 1B

Percents of each word for each function

	Subj.	DO.	OPr.	Complement
which	32.5	31.	83.	37.5
who	41.	0.	--	--
whom	--	5.2	10.5	--
that	26.	22.4	2.4	12.5
Ø	0.07	41.2	4.	50.

TABLE 1C

Percents of total number of clauses for each function of each word

	Subj.	DO.	OPr.	Complement
which	21.8	4.8	11	0.05
who	29.4	--	--	--
whom	--	0.8	1.4	--
that	18.5	3.5	0.32	0.02
Ø	0.05	6.4	0.54	0.07

TABLE 1D

Percents of each word

which	who	whom	that	Ø
39	29.4	2.2	22.3	7

TABLE 1E

Percents of each function

Subj.	DO.	OPr.	Complement
71.1	15.5	13.3	0.14

TABLE 1F

Number of occurrences of each word in each function

	Subj.	DO.	OPr.	Complement	Total:
which	1,286	266	611	3	2,166
who	1,633	1	---	-	1,634
whom	-----	45	78	-	123
that	1,030	193	18	1	1,242
Ø	3	354	30	4	391
TOTAL:	3,952	859	737	8	5,556

TABLE 1G

Average number of occurrences of each word per author

which	who	whom	that	Ø
38.6	29.1	2	22.2	7

TABLE 1H

Average number of occurrences of each function per author

Subj.	DO.	OPr.	Complement
70.6	15.3	13.2	0.14

TABLE 2A

Percents of restrictive and nonrestrictive clauses for each word

	Restrictive	Nonrestrictive
which	72.7	27.3
who/m	75.1	24.9
that	100.	--
Ø	100.	--

TABLE 2B

Percents of total restrictive clauses for each word and the percents of total nonrestrictive clauses for each word

	Restrictive	Nonrestrictive
which	34.6	57.4
who/m	29.1	42.5
that	27.4	--
Ø	8.7	--

TABLE 2C

Percents of total number of clauses of restrictive and nonrestrictive clauses for each word

	Restrictive	Nonrestrictive
which	28.3	10.6
who/m	23.8	7.9
that	22.4	--
Ø	7.1	--

TABLE 2D

Percents of restrictive and nonrestrictive clauses

Restrictive	Nonrestrictive
81.6	18.4

A Systemic-Functional Analysis of German Modality

Harold Ekart
University of Illinois at Chicago Circle

Modality in German and English poses a similar problem. In both languages the modal verbs can be used either in the epistemic sense as expressions of probability and degrees of possibility, or in the root sense to express such factual conditions on a process in a clause as permission, necessity and ability. It is because of this use of the modals for two different purposes that sentences with ambiguous semantic interpretations result. For example, in the sentences

(1) er muss vorsichtig sein/he must be careful
(2) er muss nachlässig sein/he must be careless

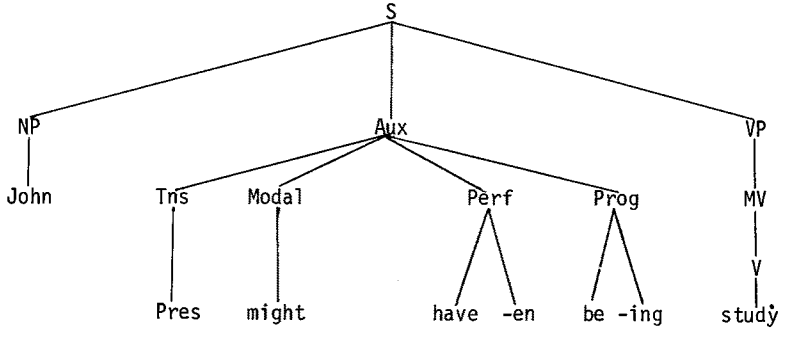
the meanings that a German or English speaker would most likely associate with the modal auxiliary are requirement (he is required to be careful) for the first pair and probability (he is probably careless) for the second. In the proper context however, the meanings associated with the modal in each sentence could be reversed. As a stage direction he must be careless could, for example, be interpreted as the actor is required to be careless.

A widely accepted analysis of the modals is presented in Chomsky 1957 and 1965 where a node Aux is postulated with the expansion in (3):

(3) Aux ⟶ Tns (Modal) (Perfective) (Progressive)
Perfective ⟶ have -en
Progressive ⟶ be -ing

In a sentence such as he might have been studying, in which the modal has only one meaning, there is a deep structure as the one in (4):

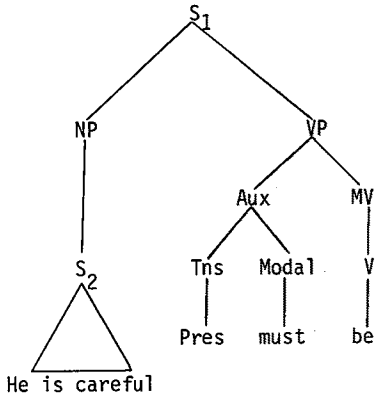
(4)



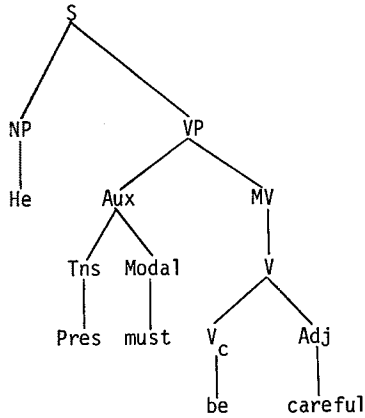
In the case of the ambiguous sentence he must be careful however, the two senses

of the modal will arise from two distinct deep structures:

(5.1) Epistemic Sense



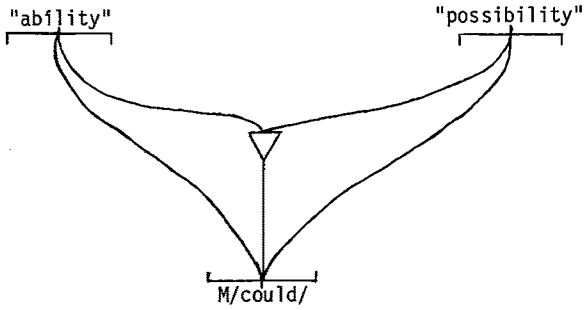
(5.2) Root Sense



Besides such ambiguities in meaning there are also instances of actual semantic blends. If we take the sentence he could have killed him if he'd tried, we'll find that any differences in meaning between paraphrases are neutralized. In the root sense this sentence could be paraphrased as he would have been able to kill him if he'd tried. In the epistemic sense the paraphrase would be it is possible he would have killed him if he'd tried. These two paraphrases are not identical, yet they are so similar in meaning that it is not necessary to choose one over the other to understand the sentence. This is in contrast to the situation in he must be careful where misinterpretation of the modal will result in the sentence not being understood correctly. It is possible to draw a deep structure for each of the meanings of he could have killed him if he'd tried. But since the situation is not the same as in the case of an ambiguity, this analysis is not sufficient as to why the epistemic and root paraphrases come to have the same surface structure. What we must therefore find is a method which will allow us to distinguish between ambiguities and actual blends in meaning.

Stratificational grammar is one such mehtod. It would account for simple ambiguity and semantic blends with a tactic diagram as below:

(6)



The upward Or node shows that there is neutralization such that the morpheme could realizes the two conceptual correlations of "ability" and "possibility." This explains a sentence such as I could still leave: it can mean either I am still able to leave or it is possible that I will still leave. The downward AND node accounts for the situation in the sentence he could have killed him if he'd tried. Here M/could/ simultaneously realizes ability and possibility.

Halliday's systemic-functional theory also distinguishes between ambiguities and blends in meaning. The subject of this paper will be to apply this theory in an analysis of the German system of modality.[1]

Before going on it should be pointed out that stratificational and systemic grammar are not mutually exclusive. Both recognize the autonomy of levels within the linguistic system. The semantic, grammatical and phonological levels that Halliday recognizes are equivalent to the strata of stratificational grammar. Both theories require that there be statements relating the categories of one level to those of another and that each level be describable on its own terms.[2]

Halliday distinguishes three generalized uses or "functions" of language. One of these uses is for the expression of the speaker's experience of the real world. This is referred to as the "ideational function." It is a factor in determining how we look at things around us and is what gives structure to our experience.

Language also serves as an expression of the communication role taken on by the speaker in a speech situation and the role assigned by the speaker to the addressee. This is the social role or the "interpersonal function" of language. It represents the speaker's choice of statement, question, response, command or exclamation in interaction with another person.

Finally, there is the "textual function." It serves to link language with itself and the context in which it is used. It is what gives a particular form to an utterance made in a specific context and is what allows us to distinguish a text from a number of sentences randomly put together.

A clause cannot be explained solely on the basis of any one of these functions. All three must be used since the meaning of a clause is derived from components of each of them. In an utterance we therefore have a speaker expressing a content, assuming a specific role in the particular situation and formulating the utterance in such a way as to make it fit the context (Halliday, pp. 325-326).

A functional analysis of the sentences he must be careless and he must be careful would be as follows. In both instances the speaker is expressing a qualification of the process in the clause. If we assume that the first sentence is a statement of probability, then the qualification resides in the speaker's own mind. The interpersonal function is involved since the qualification is related to the speaker's own communication role in which he takes up a position in regard to the thesis and gives an indication of the degree to which he considers it to be true. Taking the second sentence as a statement of requirement, the qualification resides in the circumstances and relates to a particular part of the content of the clause. Here the ideational function is involved. Unlike the situation in he must be careless, where the speaker intrudes and takes up a position, this sentence represents an expression of content relatable to the speaker's experience of the real world (Halliday, p. 347). The interpersonal is thus realized by modals interpreted in the epistemic sense and the ideational by modals meant in the root sense. Adopting Halliday's terminology, modals in the root sense will be referred to as "modulations," since their function is in effect to modulate a process. The term "modality" will be reserved for modals in the epistemic sense.

German has six modals: können, dürfen, müssen, mögen, sollen and wollen. In addition to these, werden functions as a modal when used to express probability and willingness or insistence. Their characteristics are as follows:

1) They may be used either as auxiliaries or main verbs. In a modulation the modal may be either, but in modality they may only occur as auxiliaries:

a) Er müsste kommen. (modality, modal as auxiliary)
b) Er hat es gekonnt. (modulation, modal as main verb)
c) Er konnte es machen. (modulation, modal as auxiliary)

2) When used as auxiliaries, they are in finite form while the main verb is in the infinitive: Er muss kommen.

3) Unless they are used as a main verb, they form the perfect tenses with the infinitive rather than with the past participle: Er hat arbeiten müssen instead of er hat arbeiten gemusst.

4) In sentences expressing motion, the main verb is sometimes lost through ellipsis. The modal however remains in the infinitive: Ich habe in die Schule (gehen) müssen.

5) When a modality plus a modulation occur in the future perfect, the modulation is often found in the form of a past participle: Er wird kommen dürfen/kommen gedurft haben.

6) With the exception of the forms wolle, wollet and wisse in the literary language they have no imperative.

7) Used as a main verb (ideational function) they form no passive:

a) Er mag kein Fleisch.
b) *Fleisch wird von ihm nicht gemocht.

When a modal is used as an auxiliary in the ideational function, the passive is expressed in all persons and tenses by the infinitive werden plus the past participle of the main verb:

c) Das muss er mir sagen.
d) Das muss mir gesagt werden.

In the interpersonal function the modals have no passive form. Werden never occurs in the passive voice.

8) They rarely occur as a present participle. When they do it is usually in conjunction with a verbal adjunct or an expression: wohlwollend, nicht enden wollend.

9) They may combine with each other, but more than one true modality will never occur in a clause. In the examples below, the modality is underlined once and the modulation twice:

a) Er hat ein Haus bauen wollen.
b) Er muss ein Haus haben bauen wollen.

c) Er soll ein Haus haben bauen wollen müssen.
d) *Er wird ein Haus haben bauen wollen müssen sollen.
e) Er wird kommen dürfen müssen sollen.

In his discussions on the modals, Bierwisch (1965) writes that the occurrence of one modality plus two modulations and two other infinitive forms as in c is acceptable. He rejects d in which three modulations and two other infinitives occur together. But he would consider e acceptable since only three modulations and one other infinitive occur.

10) In their conjugation, modals differ from other verbs in two ways. In the present tense, first person singular they lack the ending -e and in the present tense of the third person singular the -t: ich soll, er soll.

11) In the present tense, the singular and plural of all modals except sollen are signalled by a change in the root vowel: ich kann, darf, mag, will, muss; wir können, dürfen, mögen, müssen.

In addition to the modals, there are also lexical items which express probability, possibility and certainty. These lexical items fall into the following categories:

1) adverbs such as vielleicht, schwerlich, vermutlich, wahrscheinlich, durchaus etc. Er hat es ... gewusst.

2) adjectives used predicatively in the impersonal matrix clause es ist ..., dass ...: vermutlich, denkbar, möglich etc.

3) adjectives used predicatively in the positive or negative interpersonal, speaker-hearer matrix clause ich bin ..., dass (ob--negative form) ...: (un)sicher, (un)überzeugt etc.

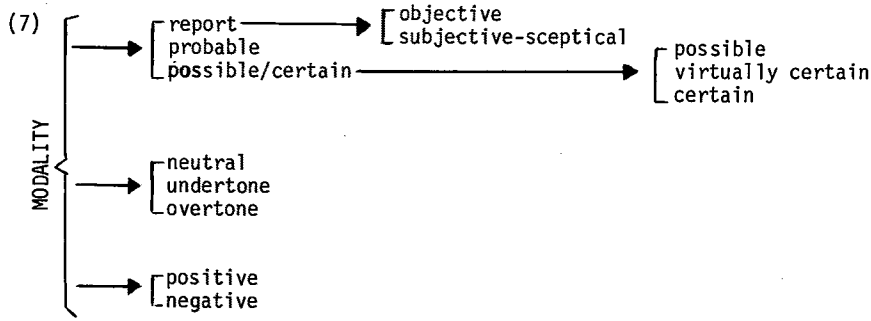
4) nouns used as a complement in an impersonal matrix clause such as es gibt/besteht die Möglichkeit/Aussicht/Vermutung dass ....

It is possible to express modality through a combination of a verbal form and a lexical item. There is however no one-to-one correspondence of a lexical item to a verbal modality. Patterns of association will probably vary from speaker to speaker, and differing nonverbal forms used together with the same lexical item will not necessarily be identical in meaning. Er hat es sicher gemacht, for example, does not mean the same as es ist sicher, dass er es gemacht hat. In the first instance the speaker is expressing virtual certainty and in the second absolute certainty. There are however groupings of lexical items and modals which are felt to be either equivalent or non-equivalent. In the first case they will reinforce each other: Er könnte es mir vielleicht gesagt haben (he possibly might have told me). In the case of non-equivalence they have a cumulative meaning, as in natürlich könnte er es mir gesagt haben (certainly he might have told me in the sense of I insist/grant it is possible).

Differing intonation patterns allow for the realization of further meanings. If er könnte es mir gesagt haben is spoken with rising-falling intonation with the tonic on könnte and the fall after gesagt, the speaker is tentatively accepting "his having been told it" as possible. When it is spoken with the same general intonation contour and tonic position, but with the fall

after könnte, then the speaker is expressing reservation.

The system of German modality is shown below in the form of a systemic network:



It is identical to the English system with the exception of the report sub-categories of "objective" and "subjective-sceptical." This is because English does not have modals in its system of modality to express what sollen and wollen do in German. Sollen permits the speaker to make a report without committing himself to an assessment of its probability or possibility of being true. With wollen the speaker can report a fact and simultaneously show he considers it improbable that it is true.

As in the other categories, the modalities of report can also be expressed non-verbally. One way is through adverbs such as angeblich and gerüchtweise: Er hat ihn angeblich gesehen. For the objective report modality, a passive impersonal matrix clause in conjunction with a verb meaning "to report" may also be used: Es wird von ihm berichtet, dass ... (it is said of him that ...). In the case of the subjective report modality, a verb meaning "to assert" plus a prepositional phrase expressing scepticism may be used: Er behauptet ohne schlagenden Beweis, dass ... (he claims without real proof that ...).

As can be seen from the network, the basic distinctions are between the categories of report, probable and possible/certain. The distinction between modalities expressing probability and possibility/certainty was determined as follows. In the latter category a contrast was found to exist between the thesis and modality negative forms:

(8.1) Es ist möglich, dass daraus nichts wird. (thesis neg.)
(It is possible that nothing will come of it.)

(8.2) Es ist nicht möglich, dass daraus etwas wird. (modality neg.)
(It is not possible that something will come of it.)

Since the two negative forms are in contrast, each requires a different verbal form. For (8.1) and (8.2) these forms are respectively:

(9.1) Daraus mag nichts werden.
(Nothing may come of it.)

(9.2) Daraus kann nichts werden.
(Nothing can come of it.)

In the probability category however, no contrast was found between the thesis negative and modality negative:

(10.1) Es ist wahrscheinlich, dass daraus nichts wird. (thesis neg.)
(It is probable that nothing will come of it.)

(10.2) Es ist nicht wahrscheinlich (unwahrscheinlich), dass daraus etwas wird. (modality neg.)
(It is improbable that something will come of it.)

Both (10.1) and (10.2) can therefore be expressed verbally as:

(11) Daraus wird nichts.
(Nothing will come of it.)

The modalities of report were found to differ from the other categories by not having a negative verbal equivalent for the modality negative. In the case of the objective report, there is no verbal form whatever for the modality negative:

(12.1) Es wird von ihm berichtet, dass er ihn nicht sah. (thesis neg.)
(It is reported that he did not see him.)

(12.2) Er soll ihn nicht gesehen haben. (verbal equivalent)

(12.3) Es wird von ihm nicht berichtet, dass er ihn sah. (modality neg.)
(It is not reported that he saw him.)

The modality negative of the subjective report sub-category also has no negative verbal equivalent. But it does have a positive one:

(13.1) Er behauptet ohne schlagenden Beweis, dass er ihn nicht sah. (thesis neg.)
(He maintains without real proof that he did not see him.)

(13.2) Er will ihn nicht gesehen haben. (verbal equivalent)

(13.3) Er behauptet nicht ohne schlagenden Beweis, dass er ihn sah. (modality neg.)
(He does not maintain without real proof that he saw him.)

(13.4) Er kann ihn gesehen haben. (verbal equivalent)
(He might have seen him.)

In (13.2) wollen is used because in the thesis negative form (13.1) the speaker is clearly expressing his scepticism about the truth of the report. In (13.3) however, the speaker negates the modality, resulting in the possibility of the report being true kept open. A modal expressing possibility in a clause with positive polarity must therefore be used for the verbal equivalent.

Halliday (p. 333) feels that the term "modality negative" is actually a misnomer since there is in fact no such thing as a statement of negative probability. A modality and a negative thesis may of course be combined, but the modality itself never enters the system of positive/negative polarity and therefore is itself not subject to negation. Negative polarity in a clause with a modal is realized only by a structural association and not a systemic relation between the negative and modal elements.

This distinction between "system" and "structure" is a main characteristic of systemic grammar. In Halliday's model for the description of grammatical phenomena, the system is characterized as a series of available choices

allowed by a language. In each language there are a number of these systems interlocking into a "system network" from which the paradigmatic relations are obtained. The syntagmatic relations, however, are obtained from the structures. The system and the structures are linked together by the fact that each available choice within the system is realized by a selection from among the structures.[3]

The complete system of German modality is represented in Table I. As in the network, the categories are set up on the basis of the distinctions that exist between the report, probability, and the possibility/certainty modalities. "Neg. I" is the thesis negative and "Neg. II" the modality negative form. The lexical items that replace or go along with the verbal modalities are shown in parentheses.

The system is partially illustrated below in paradigm form. Reading across the neutral intonation row, the sentences correspond to cells of the table.

Positive:

(14.1) Er soll ihn angeblich gesehen haben.
(14.2) Gerüchtweise will er ihn gesehen haben.
(14.3) Er wird ihn wahrscheinlich gesehen haben.
(14.4) Möglicherweise mag er ihn gesehen haben.
(14.5) Er muss ihn zweifellos gesehen haben.

Thesis negative:

(15.1) Er soll ihn nicht gesehen haben.
(15.2) Er will ihn nicht gesehen haben.
(15.3) Er wird ihn nicht gesehen haben.
(15.4) Er mag ihn nicht gesehen haben.
(15.5) Er kann ihn nicht gesehen haben.

Modality negative:

(16.1) Er mag ihn gesehen haben.
(16.2) Er wird ihn nicht gesehen haben.
(16.3) Er kann ihn nicht gesehen haben.
(16.4) Er mag ihn nicht gesehen haben.

It should be noted that the modals expressing possibility are not interchangeable. In the case of mögen and können the meanings are synonymous, but the latter is more common in the present than in the past tense. When a sentence is neutral in intonation, könnten and dürften can be used interchangeably with each other and with mögen and können. In a toned down intonation contour however, könnten and dürften are more frequent than the other two, and with overtone intonation they are the preferred forms.

An interesting parallel development in English and German centers around the modals may and the subjunctive of dürfen. Both he may come and the German er dürfte kommen are ambiguous: they may mean either probability or permission. In English might is being substituted for may to state probability, and can is replacing may as an expression of permission. Similarly, in German dürften is being restricted to expressing probability and können to express permission.[4]

To sum up, so far we have established that a distinction exists between modals used as true modalities and as modulations. A modality was determined to be a report or a statement of probability or possibility. It expresses

Table I
Modality (Interpersonal Function)

		Report		Probable	Possible-Certain		
		Objective	Subjective-Sceptical		Possible*	Virtually Certain	Certain*
		reportedly (angeblich)	allegedly (gerüchtweise)	probably (wahrscheinlich)	possibly (möglicherweise)		certainly (zweifellos)
Neutral	Pos.	sollen	wollen	werden	mögen (können/ könnten/dürften)		müssen (werden
	Neg. I	sollen	wollen	werden	mögen		können (könnten/ dürften)
	Neg. II	-	[können (könnten/ mögen*/dürften)]	werden	können (könnten/ dürften)		mögen
		reportedly (angeblich)	allegedly (gerüchtweise)	presumably (mutmasslich)	perhaps (vielleicht)	assuredly (unzweifelhaft)	obviously (offensichtlich)
Undertone: tentative, deduced	Pos.	sollen	wollen	werden	könnten/dürften (können/mögen)	müssten	müssen
	Neg. I	sollen	wollen	werden	mögen	müssten	können (könnten/ dürften)
	Neg. II	-	[können (könnten/ mögen*/dürften)]	werden	könnten/dürften (können)	mögen*	mögen
		reportedly (angeblich)	allegedly (gerüchtweise)	predictably (erwartungsgemäss)	conceivably (vorstellbarerweise)	surely (sicher)	surely (sicher)
Overtone: assertive, reservation	Pos.	sollen	wollen	müssen	könnten/dürften (können/mögen)	müssten	müssen
	Neg. I	sollen	wollen	werden	mögen	müssten	können (könnten/ dürften)
	Neg. II	-	[können (könnten/ mögen*/dürften)]	werden	könnten/dürften (können)	mögen	mögen

*möchten sometimes alternates with mögen. It is starting to be considered archaic however.
[ ] occur as positives, ( ) alternate forms

a qualification of a process related to the speaker's communication role whereby he intrudes and takes up a position in regard to the thesis and expresses his opinion of the degree to which he considers it to be true. A modulation expresses permission, necessity, inclination etc. Here instead of taking up a position in regard to the truth of the thesis, the speaker imposes a condition on a process and expresses a content relatable to his experience of the real world.

Turning now to a consideration of modulation, the fundamental difference between it and modality will be found to lie in the formation of tenses. This difference exists because modality, being outside the ideational meaning of the clause, is also outside the normal domain of tense. The paradigms below show how tense formation in modality and modulation differ. The sentence in parentheses is used to establish the context.

Modulation (obligation, modal as auxiliary)

(17.1) (Seine Arbeit ist gefährlich.) Er muss also vorsichtig sein.
(17.2) (Seine Arbeit ist gefährlich gewesen.) Er hat also vorsichtig sein müssen.
(17.3) (Seine Arbeit war gefährlich.) Er musste also vorsichtig sein.
(17.4) (Seine Arbeit war gefährlich gewesen.) Er hatte also vorsichtig sein müssen.
(17.5) (Seine Arbeit wird gefährlich sein.) Er wird also vorsichtig sein müssen.

Modality (certainty)

(18.1) (Seine Lichter brennen.) Er muss also zu Hause sein.
(18.2) (Seine Lichter haben gebrannt.) Er muss also zu Hause gewesen sein.
(18.3) (Seine Lichter brannten.) Er musste also zu Hause sein.
(18.4) (Seine Lichter hatten gebrannt.) Er musste also zu Hause gewesen sein.
(18.5) (Seine Lichter werden brennen.) Er wird also zu Hause sein müssen.

Modulation (ability, modal as main verb)

(19.1) Er kann es.
(19.2) Er hat es gekonnt.
(19.3) Er konnte es.
(19.4) Er hatte es gekonnt.
(19.5) Er wird es können.

In modality the modal appears in three forms: present, preterite and in the double infinitive construction in the future tense. The present and preterite forms are used in combination with the present and past infinitives to form the perfect tenses. In a modulation however, whether it is used as an auxiliary or main verb, the modal is subject to normal tense variation. But if modality is expressed non-verbally through lexical items, then its tense formation is also normal:

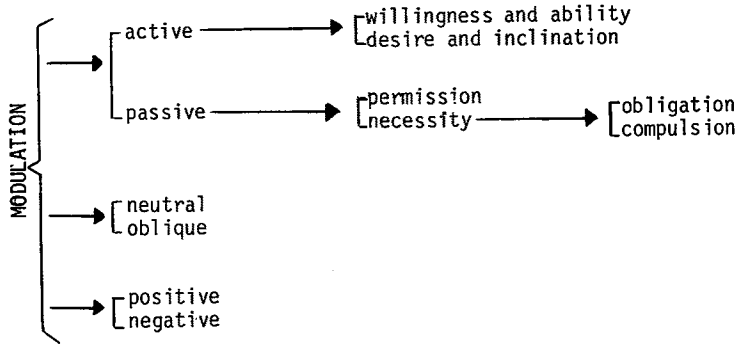
(20.1) Sicherlich sieht er ihn.
(20.2) Sicherlich sah er ihn.
(20.3) Sicherlich hat er ihn gesehen.

(20.4) Sicherlich hatte er ihn gesehen.
(20.5) Sicherlich wird er ihn sehen.

In addition to the formation of tense, modulation also differs from modality by not being expressable through lexical items. It instead has verbal structures formed by sein + adjective (er ist fähig, es dir zu sagen) and sein + passive participle (er ist verpflichtet, ins Ausland zu fahren). These verbal structures often accompany the modal verb. The result of such a combination is cumulative, as in es darf ihm erlaubt werden hinauszugehen (he is allowed to be allowed to go out).

The German modulation system is outlined in Table II and is shown in network form below. "Neg. I" is the process negative and "Neg. II" the modulation negative form.

(21)



In modality the distinction had been between the modals of the report, probable and possible/certain categories. In modulation the basic distinction is between active and passive modals. In the active type, the subject is actor with respect to both the modal and the process, as in ich kann schwimmen for example. In a passive modulation, such as ich muss schwimmen, the subject is actor with respect to the process, but goal with respect to the modal.

Within the active category there is a distinction between the modals of desire and inclination and those of willingness and ability. The former category has a non-contrastive verbal equivalent for both the modulation and process negative forms. (22.1) and (22.2) can therefore both be expressed verbally as (22.3).

(22.1) Er ist geneigt, es dir nicht zu sagen. (process neg.)
(He is inclined not to tell you.)

(22.2) Er ist nicht geneigt, es dir zu sagen. (modulation neg.)
(He is not inclined to tell you.)

(22.3) Er mag es dir nicht sagen.
(He does not want to tell you.)

The category of willingness and ability, however, has a verbal equivalent only for the modulation negative:

(23.1) Er ist fähig, es dir nicht zu sagen. (process neg.)
(He is able not to tell you.)

Table II
Modulation (Ideational Function)

		Active		Passive		
		Desire and Inclination[1]	Willingness and Ability	Permission[2]	Necessity: Obligation	Necessity: Compulsion
		Desirous (begierig)	Willing, Insistent (gewillt)	Allowed (erlaubt)	Obliged (verpflichtet)	Required (genötigt)
Neutral	Pos.	wollen	werden	dürfen/können/mögen	sollen	müssen
	Neg. I	wollen	-	müssen[3]/mögen	sollen	dürfen/können
	Neg. II	wollen	werden	dürfen/können	müssen	müssen
Oblique: hypothetical, tentative	Pos.	wollten	würden	dürften/könnten	sollten	müssten[5]
	Neg. I	wollten	-	müssten[3]	sollten	dürften/könnten[5]
	Neg. II	wollten	würden	könnten/dürften	müssten	müssten[5]
		Inclined, Partial (geneigt)	Able (fähig)	Entitled (berechtigt)	Desired[4] (gewünscht)	Designated, Intended (bestimmt)
Neutral	Pos.	mögen	können	können	sollen	müssen (haben ... zu
	Neg. I	mögen	-	müssen	sollen	haben ... zu
	Neg. II	mögen	können	können/dürfen	müssen	müssen
Oblique: hypothetical, tentative	Pos.	*	könnten	könnten/dürften	sollten	müssten (hätten ... zu)
	Neg. I		-	müssten	sollten	hätten ... zu
	Neg. II		könnten	könnten/dürften	müssten	müssten

[1]The polite form of wollen is möchten. *Oblique of mögen exists only as polite form of wollen.
[2]Here mögen is used to express permission, whereby the grammatical subject is allowed free choice: Er mag kommen, wenn er will (let him come if he wants to).
[3]A modal plus nicht is used when it is the modal which is to be negated. To negate the entire sentence, brauchen plus nicht is used. Er muss es dir nicht sagen means he is allowed not to tell. Er braucht es dir nicht zu sagen means there is no need for him to tell you.
[4]Used in the interrogative.
[5]Often used in suppositions: Nehmen wir an, dass wir in der Lotterie gewinnen. Dann müssten wir nicht mehr arbeiten. (Let's assume that we win the lottery. Then we would not have to work any more.)

(23.2) Er ist nicht fähig, es dir zu sagen. (modulation neg.)
(He is not able to tell you.)

(23.3) Er kann es dir nicht sagen. (verbal equivalent of mod. neg.)

Passive modulations differ from both active categories. They have verbal forms for the process and modulation negatives, but they are in contrast:

(24.1) Es ist ihm erlaubt, es dir nicht zu sagen. (process neg.)
(He is allowed not to tell you.)

(24.2) Er muss es dir nicht sagen. (verbal equivalent)
(He need not tell you.)

(24.3) Es ist ihm nicht erlaubt, es dir zu sagen. (modulation neg.)
(He is not allowed to tell you.)

(24.4) Er darf es dir nicht sagen. (verbal equivalent)
(He may not tell you.)

The distinction between process and modulation negative appears parallel to that between modality and thesis negative. As pointed out earlier, there is a reason for recognizing only a system of positive/negative associated with the thesis in modality. In modulation however, there are two distinct systems, one for the process and one for the modulation. It is therefore possible to have forms such as es ist ihm nicht erlaubt, es dir nicht zu sagen (he is not allowed not to tell you). Although there are also double negatives in modality, as in er kann es nicht nicht getan haben (he could not not have done it), they are associated only with the thesis. From the non-verbal form it can be clearly seen that the thesis is being negated twice: Natürlich hat er es nicht nicht getan (certainly he didn't not do it).[5]

The paradigm below gives a complete outline of how the modulations are used in the positive. Reading the table in column order, the sentences correspond to the cells in the table. The non-verbal form is shown in parentheses and requires the use of an infinitival clause.

Active:

(25.1) Er will (ist begierig,) es dir (zu) sagen.
(25.2) Er wollte (wäre begierig,) es dir (zu) sagen.
(25.3) Er mag (ist geneigt,) es dir (zu) sagen.
(25.4) Er wird (ist gewillt,) es dir (zu) sagen.
(25.5) Er würde (wäre gewillt,) es dir (zu) sagen.
(25.6) Er kann (ist fähig,) es dir (zu) sagen.
(25.7) Er könnte (wäre fähig,) es dir (zu) sagen.

Passive:

(26.1) Er darf (es ist ihm erlaubt,) es dir (zu) sagen.
(26.2) Er dürfte (es wäre ihm erlaubt,) es dir (zu) sagen.
(26.3) Er kann (ist berechtigt,) es dir (zu) sagen.
(26.4) Er könnte (wäre berechtigt,) es dir (zu) sagen.
(26.5) Er soll (ist verpflichtet,) es dir (zu) sagen.
(26.6) Er sollte (wäre verpflichtet,) es dir (zu) sagen.

(26.7) Soll ich es ihm sagen? (Ist es gewünscht, dass ich es ihm sage?)
(26.8) Sollte ich es ihm sagen? (Wäre es gewünscht, dass ich es ihm sage?)
(26.9) Er muss (ist genötigt,) es ihm (zu) sagen.
(26.10) Er müsste (wäre genötigt,) es ihm (zu) sagen.
(26.11) Er muss es ihm sagen/er hat (ist bestimmt,) es ihm zu sagen.
(26.12) Er müsste es ihm sagen/er hätte (wäre bestimmt,) es ihm zu sagen.

It should be clear that with modality and modulation we are dealing with two different systems. With modality we are in the interpersonal function where the speaker gives his assessment of probability and possibility. The system of modulation, on the other hand, is part of the ideational function. Through it the speaker expresses factual conditions on the process expressed in the clause. There are a number of differences between the two systems. To recapitulate a few of them, modals in modulation follow normal tense formation, whereas in modality they do not. Modality however allows for the formation of the passive voice, while in modulation the modals themselves are either inherently active or inherently passive. Finally, while modulations may be combined with each other, modalities cannot be.

Although the two systems are different, they are still semantically alike. The complex relationship between the two can be seen when the ambiguities which arise are considered. There are three conditions under which the distinction between modality and modulation is not clear:

(i) non-oblique + simple infinitive
(Er muss/musste zu Hause sein. (certainty vs. requirement)

(ii) oblique modal + simple infinitive
Er müsste zu Hause sein. (assuredness vs. requirement)

(iii) oblique auxiliary + past infinitive + modal infinitive
Er hätte ihn umbringen können. (possibility vs. ability

Conditions (i) and (ii) represent the instances in which modality and modulation form their tenses alike. There is no ambiguity when their tense formation differs. The third condition is a very recent development (Curme, p. 318). Previously, the combination of oblique non-modal auxiliary and the simple and modal infinitives had been limited to expressing modulations. As a modality, the sentence in (iii) used to be formed with the oblique modal plus the past infinitive plus the infinitive of sein or haben: Er könnte ihn umgebracht haben. Although this form still occurs, the combination of elements in (iii) is now generally used to express both modality and modulation.

Blends in meaning result when sentences ambiguous under (ii) and (iii) are put into hypothetical environments. The sentence er könnte kommen, wenn das Wetter schön bleibt (he could come if the weather stays nice), for example, can be paraphrased either as he might come if the weather stays nice or he would be able to come .... Similarly, hätte er es versucht, hätte er ihn umbringen können (he could have killed him if he'd tried) can mean either "he would have been able to" or "he possibly might have." As is characteristic of blends, these sentences will be understood correctly regardless of whether the hearer choses the modality or modulation paraphrases as the meanings.

Now we are getting close to the point of this paper: how are these ambiguities and blends to be explained? If the systemic networks for modality

and modulation are compared, one can see that the two systems look different yet match closely term for term. Although it is difficult to determine exactly what the relationship is, the notions of "probable" and "willing," and "possible" and "permitted" are in some sense equatable. The first relationship can be explained as follows. If a student says I will probably study to pass the exam, then he is most likely also willing to study in order to pass. Likewise, there is also a relationship between permitting something and thinking it possible (Halliday, p. 349). For example, if someone permits me to do something, then he probably thinks it possible that I am capable of doing it.

A similar relationship holds for the categories of "subjective report" and "desire." If we look at the sentence er will ein guter Christ sein (he wants to be/claims to be a good Christian), it becomes clear that there is not only ambiguity, but also a likeness in meaning between will as a modality and as a modulation. This is because there is some similarity between claiming and desiring something. With sollen in the categories of "objective report" and "obligation," there is also this ambiguity and similarity in meaning. The similarity lies in the fact that when a person states something about someone, he is not far from obligating it of him: Der Pfarrer sagt, Hans soll ein guter Christ sein (the priest claims John is/wants John to be a good Christian).

This also holds for the categories of "certain" and "compelled." If we compel someone to do something, then it is usually fairly certain he will do it: Er muss vorsichtig sein (he is required to be/is certain to be careful).

The relationship between the systems of modality and modulation can be shown through "content-substance" grammatical labels (Halliday, p. 346). Using these labels and disregarding the sub-categories not identifiable across systems, the two systems can be reduced to a single combined modality/modulation network:

(27)

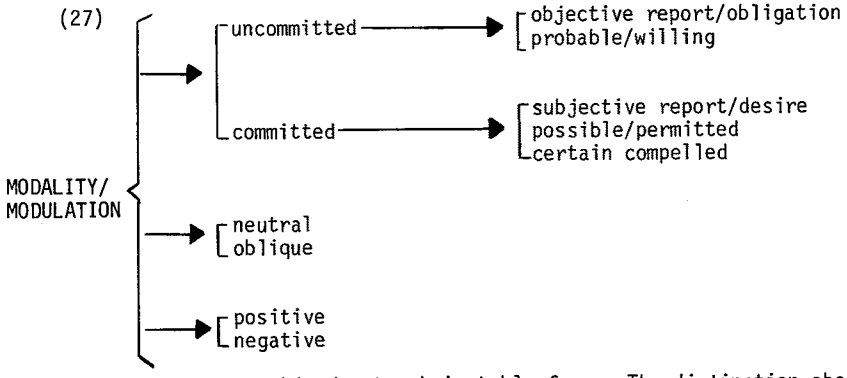


Table III shows this combined network in table form. The distinction observed is between the modals of the "uncommitted" and "committed" type. This distinction will become clear through the following examples. In the objective report modality, the speaker is "uncommitted" since he does not take up a position in regard to the truth of the report. This is in contrast to the subjective type in which he commits himself to a statement of improbability by expressing his scepticism. Similarly, if one is obliged to do something, one is not really committed to do it. However, when one is compelled, then one is in effect

Table III
Modality/Modulation

		Uncommitted		Committed		
		Objective Report/Obligation	Probable/Willing	Subjective Report/Desire	Possible/Permitted	Certain/Compelled
Neutral	Pos.	sollen	werden	wollen	können	müssen
	Neg. I	sollen	(werden)	wollen	(mögen) [müssen]	können
	Neg. II	[müssen]	werden	({mögen/können}) [wollen]	können [dürfen]	(mögen) [müssen]
Oblique: tentative, hypothetical etc.	Pos.	[sollten]	[würden]	[wollten]	könnten/dürften	[müssten]
	Neg. I	[sollten]	-	[wollten]	[müssten]	dürften/könnten
	Neg. II	[müssten]	[würden]	[wollten] ({könnten/dürften})	könnten/dürften	[müssten]

Neg. I: Thesis or Process Negative
Neg. II: Modality or Modulation Negative
( ) modality only, [ ] modulation only, { } occur as positives

committed.

Although the two systems are reducible to one network, they are not identical. This phenomenon of "same but different" is nothing new to linguistics and is accounted for by the concept of realization. In this instance it is the functional diversity of language which lies at the basis of this phenomenon (Halliday, p. 347). With modality and modulation we have to do with the same system used in two different functions, the interpersonal and the ideational. In both cases we are concerned with the qualification of a process in a clause. The system is entered from the interpersonal function when the qualification is in the speaker's mind. Here the declarative mood is tempered by the speaker's assessment of the probability or possibility of the declaration being true. If the qualification resides in the circumstances, the system is entered from the ideational. In this case a modulation accompanies the transitivity structure to express the conditions which modulate the process. Ambiguity results when the context and tense formation do not make it clear to the addressee from which function the speaker entered the system.

Overlap between the two systems in conjunction with identical tense formation account for blends in meaning. In the passive type of modulation, i.e. the modulations of permission and necessity, the qualification does not come from the participant. In the sentence er muss vorsichtig sein, for example, er is the agent of the process, but goal of the modulation. The agent of the modulation is the speaker, for it is in the speaker's opinion that there is a qualification on the process. The clause is then not a characterization of the actor's participation in it, but of the speaker's judgment of the actor's participation. This makes it very close to a modality. A modulation, especially one of the passive type, could therefore be described as follows: it is a condition imposed by someone, but if the person who imposes the condition is the speaker, then the modulation becomes a type of modality, a "quasi modality" as Halliday terms it. Modulations are thus part of the ideational, but represent that part of it that is oriented towards the interpersonal.

In addition to the ideational overlapping the interpersonal, the interpersonal also overlaps the ideational. In modality the speaker intrudes and takes up a position, showing the validity he attaches to a declaration. It is part of the attitude taken up by the speaker, actually the speaker's attitude towards his own speech role, and is external to the content. This so far falls within the interpersonal, but it is also oriented towards the ideational since an attitude towards the content is being expressed.

Besides their formal likeness which results in ambiguity, one other area of similarity between modality and modulation is then this semantic region of speaker's commentary on the content where the interpersonal and the ideational overlap. The interpersonal includes all types of commentary by the speaker. When the commentary relates to the probability of content, the interpersonal is expressed through the syntactic system of modality. The ideational includes all aspects of content. Those that involve the speaker are expressed through the syntactic system of modulation. The two functions therefore overlap each other, sometimes resulting in blends in meaning (Halliday, pp. 347-350).

Footnotes

[1]This analysis of German modality is based largely on an article by M.A.K. Halliday, "Functional Diversity in Language as seen from a Consideration of Modality and Mood in English." Foundations of Language, 6 (1970), 322-361.

Page numbers referring to this article will be given in parentheses within the text.

[2]David G. Lockwood, Introduction to Stratificational Linguistics, (New York et al.: Harcourt Brace Jovanovich, 1972), p. 260

[3]Ibid.

[4]Herbert L. Kufner. The Grammatical Structures of English and German, Contrastive Structure Series, (Chicago and London: University of Chicago Press), p. 37.

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Acknowledgements

I would like to express my thanks to Professors Halliday and Ádám Makkai for their suggestions and comments on an earlier version of this paper.

PRIMARY VERBS

Charles Ruhl

Old Dominion University
Norfolk, Virginia

The fundamental assumption of modern linguistics is that language is systematic. This is a holistic view, in which the distinctions within the system are defined as divisions of a pre-existing whole. An element in a system is defined by its mutual contrasts with other elements in the system. In the strictest definition of system, elements which appear identical in isolation can not be defined as identical if they differ in contrast partners within their respective systems. For example, the /p/ and /t/ of a phonological system with no other voiceless stops can not be strictly identified with the /p/ and /t/ of a system which also has /k/. In the first system, /p/ is defined as not-/t/; in the second system, it is both not-/t/ and not-/k/. In actual practice, the definition of system is much looser, and elements are identified across systems which are not strictly identical. In determining the vowel phonemes of English, for example, the vowels which occur before stops are identified with those that occur before /r/, even though there are less vowel contrasts before /r/. There are also some situations in which we can have different systems and yet have identical elements in both systems. Such is the case of two systems, one of which has Singular, Dual, and Plural morphological indicators and another which has just Singular and Plural. Since Plural in the second system fills the role of both Plural and Dual in the first, the role of Singular is the same in both systems.

My concern in this paper is with a more complex situation where it seems profitable to identify elements which belong to different systems. Suppose we have in a single language two systems, one of which is a subsystem of the other, as in (1-2). In a certain domain -- where a domain implies a set of contexts -- the elements in (1) exhaust all the possibilities, thus creating a three-way contrast; in another domain, the fuller set in (2) is required to account for all the possible contrasts. The

(1) lik,fot,rud

(2) lik,fot,rud,hum,vit,slaf,cobor,rimarta,simoka

question is: how are the sets related? In strict systematic terms, the elements in (1) can not be identified with their counterparts in (2) because the contrast possibilities are different. Nevertheless, it may be possible -- and eventually necessary -- to equate them under two conditions: first, if the relationships of lik, fot, and rud to each other are comparable within the two systems; and second, if we can consider these three elements to be the "primary" elements in this set of elements, where under a general linguistic condition primary elements are those elements of a set which are used in domains where contrasts are limited. We have a very clear example of such a case: the number system. The positive integers form a primary set of numbers, on which the negative integers, fractions, etc., are elaborations. In addition, we can also consider the round numbers -- 10, 20, 30, ...; 100, 200, 300, ...; etc. -- even more primary, since they are used in limited domains where only round-number information is needed. In strict systematic terms, these would all be

different systems; but mathematical relationships between the elements are constant, no matter how limited a part of the system is used, and so our two stipulated conditions hold.

A crucial distinction must be made in this discussion between extensional and intensional definitions -- that is, between an element's reference and its meaning. Although references of a set of elements may have some degree of systematicity, there is no guarantee or requirement that they should; the notion of system in linguistics has always been intended to apply intensionally. That is to say, the systems that linguistics studies are not a property of the external world, but of the human mind. I hope to show in this paper that the full implications of this intensional orientation have not been grasped, at least in the study of semantics. Of course, the intensional orientation is very clear with mathematics. If we defined the number system extensionally -- that is, according to our intended reference -- then we would have different subsystems: in instances where we round off, 100 may refer to anything from about 95 to 105; in more elaborated situations, it may mean anything from about $99\frac{1}{2}$ to $100\frac{1}{2}$; and in still more elaborated situations, it may mean precisely 100 and nothing more or less.

(We may note, incidentally, that the existence of subsystems in the numbers creates an ambiguity in the definition of the word number. The ambiguity can be exploited by the type of trick -- recently demonstrated in a Blondie comic strip -- where you ask people to guess a number between 1 and 10, and when everyone fails to guess it, you reveal the number was $6\frac{1}{4}$. However, the presence of an ambiguity does not mean that the word number itself is ambiguous. In a complete linguistic theory, the word number would designate the whole multi-system, and would be domain-free; the context in which it was used would specify or imply the domain, and the combination of the word-meaning and the domain-conditions would delineate which part of the number system would be used. The "least-marked" context -- in computer terminology, the "default" option -- would be, for the numbers, the positive integers).

The need for intensional definition is obvious with the numbers, but it is less so when we turn to another multi-system with similar properties: the system of colors. At issue here are a number of apparently "idiomatic" uses of color terms, as with white, black, and red in (3-5).

(3) white skin, white wine
(4) black skin
(5) red skin, red hair

Such uses seem to require that a term such as red must be treated as polysemic. Since a polysemic treatment is less preferable than a monosemic one -- which would supply an additional generalization about the language -- we must ask if we can avoid such a conclusion. We begin by noting that there are colors which are considered primary, both by scientists in color theory and by any speaker of English; red is such a primary color, while maroon is not. The research of Berlin and Kay (1969) supports this conclusion. They claim that the most primitive color system has only a two-way color distinction, that between white and black; if a third color is added, it is red. Of course, in such rudimentary systems, the extensional values of the terms we would gloss as white, black, and red are much broader in range than the extensional values they have in our more elaborate system; but Berlin and Kay mean that the systematic relation-

ships are the same in each system. Consider that the domains involved in (3-5) -- skin color, hair color, wine -- are domains of limited color distinction. The limited domain makes fewer contrast partners necessary, and this has the effect of enlarging the extensional range of each primary color, just as in the rudimentary systems. Since red is the finest distinction that needs to be made with respect to hair in a certain area of the color system, it is applied to a color which could not be called red in a more elaborate system. What is puzzling and seemingly contradictory in extensional terms is perfectly systematic in intensional terms. A polysemic treatment of red focuses too much attention on its extensional range. Such a treatment leaves unexplained why the word is used in strikingly different ways, and so the intensional analysis is clearly prferable.

(Any domain, of course, can include domain-specific words like blonde. Significantly, the nickname "Whitey" is sometimes applied to blondes. The hair color we call white is a derived color, produced by aging; in the primary system of hair colors, blonde hair could assume the position of white. Etymologically, the term blonde means 'white'. What I said earlier about the word number also applies to the word color. In different domains, color varies between 'white but not black', 'black but not white', and 'neither white nor black'; but these variations are attributable to the domain, and not to the word color, which is domain-free.)

A number of crucial implications follow from such an analysis of color terms. First of all, primary words are highly abstract -- which is another way of saying that they are completely domain-free. Since they are the most intensional of words, their connection with extensional uses is highly remote. In fact, any extensional value they acquire in some context is likely to be highly deceptive. It must be stressed, however, that the abstractness of primary words is a systematic abstraction, not an atomistic one; when we usually conceive of abstractions, we do so one concept at a time, and thus most of our deliberate abstractions tend to be atomistic. For example, someone who insisted that red hair should be called orange or bronze or the like would be insisting on an atomistic abstraction. Typically, those abstractions which we make deliberately are systematic only when they can be correlated with mathematics. The uniqueness that mathematics seems to possess may be attributable to our high degree of ignorance about other intensional systems. (Implicit in these comments, you may have noticed, is the assumption that we do make systematic abstractions nondeliberately -- that is, subconsciously. More about that later.)

Second, because they are domain-free, primary words are impossible to paraphrase. A proposed paraphrase --typically, a word which is more specific, and more domain-specific -- will both overspecify and undergeneralize. (That is, in transformational terminology, it will be both "too strong" and "too weak.") In a particular context, a paraphrase may in fact be accurate, but only because the differences have been cancelled. The paraphrase will be more specific than the primary word, but the context may allow or require the primary word to take on a more specific meaning. On the other hand, the other uses of both the paraphrase and the primary word will not be relevant in the particular context. These are familiar facts to all linguists, but they must be specifically noted, and

not merely assumed, in a linguistic climate where paraphrase has an inflated value.

Third, as already noted, the apparent meaning of a primary word is highly variable in context. The extension it assumes may often be totally the contribution of the context and its related domain; the best example of this is the verb take, which I will discuss shortly. As in the case of red, the different extensions of a word may even seem contradictory. There is a word of caution here for any method of semantic analysis that relies too heavily on contextual -- that is, syntactic -- criteria. Of the two linguistic dimensions, the syntactic and the paradigmatic, the former is more tied to extension; words that are wholly intensional in nature, the primary words, must be studied mainly on paradigmatic criteria. Yet, our knowledge of intensional relationships is meager in comparison with our syntactic knowledge. As a result, our methods are heavily biased toward syntax and extensional information; our major tool for determining paradigmatic relationships is a syntactic one: the contextual method. But context is even more mysterious than a single term, since it is naturally more complex. Language is a system of many interacting systems, and the more complex a structure, the more likely it is to be influenced by many conflicting factors which distort or cancel each other out. Transformational Grammar has vividly demonstrated this fact. It is even possible for factors not normally considered to be linguistic to influence structures. I discussed one such case in Ruhl (1973). The UCLA Syntax Project noted that there was variation in the use of pronouns with three bisexual species: for horses, he/she/it is possible, for humans he/she but not it, and for fruitflies it but not he/she. Using strict systematic criteria, the Project developed three sets of pronouns with different features, the features being indicators of contextual possibilities. I suggested that the variation in pronoun use came not from different pronoun systems, but from an independent principle that had the effect of distorting the permitted patterns: the principle that the higher in the order of animals (with an order of animals independently specified), the more necessary it is to make sexual distinctions. Except for babies (who are not fully human in social terms), it is always necessary to make distinctions with humans, but optional with horses, and unncessary or impossible with fruitflies. The Project's analysis shows how far a syntactic criteria can distort, for in suggesting that there were three different pronoun sets they denied rather clear semantic consistencies. In cases where the meaning of a term appears more variable, it is even easier to reach a polysemic conclusion.

Ideally, we should start with the set of contrasting elements securely in hand, and use them to weave through the many torturous paths of syntax. But, as I noted, such information is even more remote than syntactic information. For the reasons just noted, it does not seem promising to proceed by the contextual method, even though that appears to be the only method available. However, an alternative approach is to deal exhaustively with those words which exhibit the properties which I have just attributed to primary words: their systematic abstractness, their resistance to paraphrase, and their high variability of contextual meaning. Any such word will appear to be highly polysemic. With this rather lengthy preparation, I now turn to the major topic of my paper, some of the most common and elementary verbs in the English language -- listed in (6) -- which exhibit

all these characteristics and which I will thus call "primary verbs." The

(6) be, do, have, get, take, give, come, go, put, make, hit, break, cut

first three verbs are the most primary of all, so much so that they appear to be merely syntactic markers, with little or no inherent meaning. When they do acquire some substantial meaning, it is either the effect of context -- as in the sense of 'equal' in be, as in Three and two is five -- or the effect of an enlarged set of contrast partners. This latter possibility can be illustrated with do. On one level, do contrasts only with be, representing in part the distinction of verbs and adjectives. In this use, do acts like a modal, occurs before negation, and is considered a periphrastic element, empty of meaning, that can co-occur with any other verb (except modals); the contrast with be is illustrated in (7-8). In a more elaborated system, involving contrast with verbs like feel, the range of do is limited only to active verbs, it occurs after negation, and it seems to acquire an active sense. This use and a contrast is illustrated in (9-10). (These observations are only suggestive,

(7) Bob wants to go, but Jim doesn't.
(8) Bill is eager to go, but Jack isn't.
(9) Ben demanded to go, but John didn't do that.
act that way.
*feel that way.
(10) Bob wants to go, but Jim doesn't feel that way.
*do...

drawing on the conclusions reached in Transformational analyses. I don't

(11) Carl towers over Ted, but Fred isn't that tall.
doesn't.
doesn't come up to his waist.

want to make the latter system seem too simple; stative verbs can have diversified patterns as in (11).) In more specific contexts as do the laundry and do your homework, do can seem even more specific. Once we factor out contextual information, which creates the more specific variants, these verbs appear to be purely syntactic. This leaves open the question of what a purely syntactic role is. The correlation of do and be with verbs and adjectives is assumed to be arbitray, because we have no consistent semantic distinction to apply to it. But the distinction may seem arbitrary simply because it is so primary and intensional. I'll return to this point later, when I hope to make it more comprehensible.

Actually, the worst examples of polysemy are the rest of the verbs in (6), which are slightly less primary, and thus have more of the "semantic stuff" that we can pin down extensionally. A major analytic problem with these verbs -- and a number of others -- is that they range over the Concrete/Abstract distinction and also have a large number of "metaphorical," "idiomatic," or "empty" senses. A crucial question for semantics is whether to (a) make such distinctions with these verbs, with the result that they are polysemic, or (b) treat the verbs as monosemic -- covering even metaphorical contexts -- with the result that the distinctions apply only in a limited part of the grammar. (It is, by the way, a measure of the semantic emptiness of be, do, and have that such a dilemma is never even considered with respect to them, even though they have the same property.) In practice, the former option -- to extend the Concrete/Abstract distinction and make the words polysemic -- has been the path taken by Transformational Grammar. In general, any theoretical orientation

that views language as autonomous, a psychic entity complete in itself, tends to take the polysemic path. Such an orientation, which in its extreme form equates natural language and artificial language, isolates linguistic competence from other psychic competence. It thus commits an error which seems to me to be especially typical in the "academic" mind: it tends to impute to all of the language the characteristics of the most conscious and rational aspects of language. Every question is approached and solved in terms of what is comprehensible to rationality. A distinction which can not be explained by conscious thought -- such as that between verbs and adjectives -- is defined as purely syntactic and arbitrary. A generalization that can not be grasped consciously -- such as a single meaning for take -- is simply denied. Conversely, the rational distinction between literal and metaphoric is considered fundamental, applicable throughout the grammar, and metaphor can then be safely ignored. As will be increasingly evident, this "academician's error" is what I am really stalking in this paper.

The form this error takes is what the philosopher Whitehead has called "misplaced concreteness" and what Bolinger (1971) has called "semantic overloading"; in general, it is the typical error of conscious thought, that of overanalysis. The verb break serves as an illustration. As analyzed in various Transformational treatments, break is ambiguous in at least three ways, as exemplified in (12-15): the Transitive/Intrans-

(12) John broke the glass.
(13) The glass broke.
(14) John broke the truce.
(15) *The truce broke.

itive distinction, as in (12,13), in which the former has a sense of causation which the latter lacks; the Abstract/Concrete distinction, as in (12,14), where the non-parallelism of (13,15) provides supporting evidence; and the Deliberate/Nondeliberate ambiguity of (12), where John can have broken the glass by intentional action or by accident. This last ambiguity is supported by the fact that in (16) both breakings must be

(16) John broke a glass, and Bill did too.

deliberate or nondeliberate. (It is claimed that the interpretations can not be mixed, although the sequence nondeliberate,deliberate -- in which Bill is consciously imitating everything John does, some of which may be done accidentally -- seems possible to me.) As (17) shows, it is not al-

(17) The news broke.

ways true that abstract nouns can not occur with intransitive break; thus, we have, with these three distinctions, eight possible lexical items, if the distinctions are not generalized. Such a high degree of polysemy, while possible, seems unwarranted here, and generalization seems the proper course. But if an element can occur with both parts of a two-way distinction, then it is in fact neutral with respect to that distinction, and a feature is not necessary in its semantic description. The extravagant use of features in Transformational analyses obscures the purpose of specifying a feature in a word's lexical entry: the purpose of noting a limit on the word's range. But the only limit on break in the above sentences is with respect to an intransitive subject truce, and that limitation may be in the noun, as (17) suggests. In addition, the senses of causation and deliberateness -- and of noncausation and nondeliberateness -- can be inferred, respectively, from the transitive structure and from

the potential abilities of an entity called John. I say "infer" deliberately, because I doubt if even the sentence (12) is ambiguous. It can take either a deliberate or nondeliberate analysis, but it can also take an analysis that is intermediate between these two extremes, where the degree of deliberateness in John's action is indeterminable or variable in degree. Deliberateness and nondeliberateness are the polar extremes of a continuum; and even if no continuum existed, the sharp distinctiveness we bring to sentence (12) is attributable to the polar evaluation we impose upon it, and not to the sentence itself. The supposed ambiguity of break or of sentence (12) as a whole thus derives from our manner of interpreting, a manner which explains the ambiguity; and by duplicating this information in the word or sentence we needlessly complicate the grammar. This is a typical case of what I mean by "overanalysis."

An approach committed to rampant polysemy would produce even more havoc with the verb take, which is the most polysemic word in most dictionaries. Sentence (18) can take a wide variety of interpretations, as the use of adverbial modification in (19-25) demonstrates. (We could use adverbial modification in the previous discussion of break as well.)

(18) John took the medicine.
(19) John deliberately took the medicine.
(20) John accidentally took the medicine.
(21) John helplessly took the medicine.
(22) John illegally took the medicine.
(23) John willingly took the medicine.
(24) John absentmindedly took the medicine.
(25) John took the medicine with him.

Modification of other elements is also possible, with further shifts in meaning, as in (26-28). Additional possibilities can easily be added.

(26) John took the medicine in preference to getting sick.
(27) John took the unwanted medicine.
(28) A badly beaten John took the medicine with little resistance.

The set as a whole not only reveals a wide range of semantic possibilities, but each sentence serves to reveal possible ambiguities of interpretation in all the others. Ironically, even if one intended to overanalyze, the most fruitful method would be to deal with take as if it were a unified word. The range of possible paraphrases is quite diverse: an active sense of 'steal' in (22), a passive sense of 'endure' in (21) -- note also take a lot of punishment -- a sense of movement in (25), a sense of acceptance in (23) that can be either active or passive, a sense of chosing in (26). Yet, sentences (18-28) account for only a small part of the range of take. Variation in the object of take shows physical, mental, emotional, and communicational possibilities, as in (29-32). Such dis-

(29) John took the book.
(30) John took it that Bob was sincere.
(31) John took criticism well.
(32) John took the hint that he should go.

tinctions are vital in rational schemes, but take seems oblivious to them. Also, take seems most naturally in conjunction with the preposition from, in mutual contrast with give to; under this interpretation, the subject of take usually takes on the role of Goal. But this is not always the case, as some sentences with the preposition to demonstrate, as in (33-43).

(33) John took to Mary.

(34) John took a liking to Mary.
(35) John took to swimming daily.
(36) John took to the hills.
(37) John took flight to the hills.
(38) John took to swimming as a duck to water.
(39) John took a blow to his chin.
(40) John took an ax to the tree.
(41) John took the bus to Chicago.
(42) John took a walk to the drugstore.
(43) John took off to the drugstore.

In (42), we have a relatively "empty" use of take, which is probably such because of the lack of any other verb as contrast; in this context, take fills the whole semantic range. Sentence (43) is only one example of a great variety of verb-particle constructions including take. There are also many other examples of seemingly distinct senses of take, including such constructions as take offense, take aim, take comfort, and the phrase which seems most idiomatic of all, take place. As new examples are added to the list, the increasing polysemy gradually yields to a sense which is very general and abstract, and also unified. As long as one's attention is fixed on a particular example, the extensional differences loom large; a large body of data serves to neutralize these misleading differences. As my ongoing research on take has demonstrated to me, it requires an immense amount of data even to suspect what a fully intensional system might be.

One brief example can serve to show how apparently significant differences can be eliminated. Sentence (41) shows take with an apparent sense of movement; however, sentences (44-45) (the first due to Fillmore (1974), with the observation made here) show that the sense of movement can derive solely from the prepositional phrase and that the verb need

(44) She wore a mink coat to the party.
(45) He invited her to dinner.

not involve movement. A proper understanding of movement with take also involves concurrent analysis of other apparent movement words. In various contexts, take contrasts with bring, give, and put, all of which seem to involve movement. Yet, for these verbs -- and come and go as well -- a sense of 'physical movement' is too specific if we treat them as words with unified meanings. As is the case with systems, a proper analysis of one word requires a consistent analysis of its contrast partners, and all of these verbs are highly abstract and intensional.

It can be objected that these claims run counter to a speaker's very clear intuitions that a physical sense is basic to all these words, and that other senses are derivative. It is true that physical senses tend to be more accessible to conscious awareness, and that these senses even tend to have the "least-marked" contexts. However, we must ask if this prominence of physical senses is a property of the verbs themselves, or is instead attributable to general processes of human perception, which tend to make physical and active senses more immediate and easier to notice. Other senses would then be more subconscious, less accessible and less noticeable. In general, primary words are rarely noticed; we use them without thinking, with only minimal phonological prominence, and as a result they come out in phrases so stereotyped that they are almost cliches. Only with the active physical senses, or the intense emotional ones,

is there any significant conscious awareness or phonological prominence, and this fact creates the impression that these are basic senses.

All of this may seem remotely plausible, but we need to ask a basic question: why should there be primary verbs at all? We can couple this question with other linguistic facts which at first glance may seem unrelated. Languages have a broad contrast of two kinds of elements, one which belongs to large and potentially open sets and another which belongs to small and closed sets. This is the distinction between major and minor word classes, between lexical and grammatical morphemes, and between "full" and anaphoric forms. In general, this is the distinction between elements which appear to have meaning and those which are merely syntactic. Actually, the distinction is not so polar as this description implies; while there are some prepositions with purely syntactic roles -- such as of -- there are others which have meaning. In addition, the class of prepositions, while not as open as the class of nouns, is not as completely closed as, for example, the pronouns. And, as my discussion here implies, there are verbs which are relatively empty of meaning, and these verbs comprise a set which is relatively closed. In short, there is evidence that suggests a continuum of systems in language which range from the large and open systems to the small and closed systems. Although it is impossible to demonstrate at this time, I suspect that the alleged discrete split between syntax and semantics -- the first of which is much more constrained, much more closed, in contrast to the latter -- is instead also a continuum, or rather, is merely another aspect of the same general continuum.

What accounts for such a continuum? Why should there be open and closed systems in a language? To answer this question, I would begin by arguing that language is not an autonomous psychic system, and like other psychic phenomena it exhibits variation that parallels the psychic continuity from conscious to subconscious to unconscious. In general, conscious systems are more diversified and open, while the "primary processes" of the unconscious are less diversified and closed. The unconscious provides the basic psychic order; it is unconscious simply because it is taken for granted, its systematic dictates no longer open to conscious questioning or alteration. The conscious mind, on the other hand, provides the possibilities of freedom, and it is more open to change, simply because it is aware. In parallel, the closed systems of a language provide the little-questioned order which that language imposes on the world, while the open systems provide the creative possibilities. It is a basic fault of the conscious mind to deny or minimize the order the unconsciousness provides; but as any unreadable stretch of Latinate English prose demonstrates, the unconscious strata of the English vocabulary is badly needed, and it consequently accounts for a major part of the words that occur in English sentences. The inflectional system in particular is a repository of the most basic assumptions in the language. These barely physical cues perform the same function of solidarity that the kinesic systems noted by Birdwhistell (1970) do. Significantly, pidgin languages have no inflectional systems; their speakers have no established beliefs in common, nor do they desire any. As pidgins change to creoles, and the need for solidarity arises, inflectional systems develop.

Linguists are aware that much of our linguistic knowledge is sub-

conscious, and that a person knows consciously very little of what he knows subconsciously. The crucial question here is: what do we assume about that subconscious knowledge? Do we assume -- whether it is innate, habit formation, and otherwise -- that it can be readily understood and described in conscious terms? Or, rather, do we consider that subconscious knowledge operates on principles that are hard for the conscious mind to grasp. There is evidence that primary psychic processes pay no attention to concrete-abstract or literal-metaphoric distinctions, that they are heedless about time and space dimensions, or even about the unity of events or objects. Can the conscious mind follow such logic? If it is true that the conscious mind finds such logic difficult, we have a ready explanation for a speaker's typical misunderstanding of what he knows: he is continually analyzing his knowledge in the atomistic -- that is, the "open" -- and extensional modes of consciousness rather than the systematic -- "closed" -- and intensional modes of the unconscious. Thus, the academician's error I mentioned earlier -- the tendency to explain everything with the categories of consciousness -- is simply a more principled version of a common human misunderstanding. The error in fact does us considerable harm, making us seem less than what we are, just as the general expression of an exclusive consciousness -- the mechanistic view of life -- also belittled us. With the new ecological view of life, the systematic view of life, the conscious mind is more humble, more aware of the subconscious order that underlies it and makes it possible. And such insight gives us a sense of a more complex and resourceful self than an exclusive consciousness ever gave us. It seems likely to me that the failure of linguists to understand the full intensionality of primary words has had a restraining effect on the visions of anyone who has pondered human capabilities. Properly understood, words like take and break should fill us with awe at the capability of the human psyche. To make possible that sense of awe may be a linguist's principal goal.

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VII
Stratificational Solutions to Some Old and Some New Problems

Quasi-Etymological and 'Natural' Phonology as Two Varieties of the Same Mistake

David G. Lockwood
Michigan State University

1. Quasi-etymological phonology.

The term 'quasi-etymological' is used in this paper to refer to the practice of certain transformational-generative phonologists of postulating underlying phonological structures bearing a strong resemblance to the surface structures assumed for a much earlier, and generally quite distinct, period in the history of the language concerned. This approach is particularly well exemplified in some M.I.T. dissertations of the mid-1960's, especially Lightner's on Russian, whose main conclusions have been published in Lightner 1972, and Schane's on French (Schane 1968). A similar general approach is applied to English in Chomsky and Halle 1968.

It must be conceded, of course, that any morphophonemic transcription is bound to resemble an earlier stage of the language to some extent, since morphophonemic alternations result from historical changes. The systems postulated by followers of the quasi-etymological view, however, resemble histories to such a degree and in such a depth that it is often difficult to see how they can be considered valid characterizations of the phonological competence of the average speaker. As a case in point, let us consider Lightner's resuscitation of the ancient Slavic phonemes termed jers. According to the more conventional Slavic linguists, there jers were extremely lax high front and back vowels which existed in Proto-Slavic (often termed 'Common Slavic') and in such attested languages as the oldest Old Church Slavic and Old Russian, but which later ceased to exist in the various Slavic languages due to loss in certain (so-called 'weak') positions, and merger with other vowels in the remaining (so-called 'strong') positions.[1] According to these conventional treatments, the weak jers were lost in Old Russian during the Twelfth Century, and the strong ones merged with other vowels soon thereafter. Lightner claims, however, that these earlier Slavic linguists were incompetent coroners: they pronounced the jers dead long before their time. He has discovered that they are still alive in Modern Russian, albeit at a deeper level of existence, but still possessing the same sort of phonetically interpretable representation which they had in Proto- and even Pre-Slavic: short high vowels, front and back (Lightner 1972:33-4).

Lightner, like others of this school, constantly furnishes his readers with purported justifications for his conclusions in the form of synchronic data which they are intended to explain. In many such cases, however, one is left in serious doubt that the speaker, without the conscious historical knowledge of the linguist, could actually arrive at such a sophisticated treatment purely from the application of his language-learning ability to the data of the language during the process of language acquisition. The quasi-etymological approach is thus open to criticism on the ground that it assumes a vastly greater ability on the part of the naive speaker than can be adequately demonstrated to exist.

2. 'Natural' generative phonology.

In the mid-1960's the quasi-etymological approach was the only widely accepted version of transformational-generative phonology. By the 1970's, however, there has arisen a competing trend generally known as 'natural phonology'. The naturalist trend appears not to be a single theory, but rather a cluster of related ones all sharing certain basic principles with Chomskyan doctrine, but diverging from it, as well as from one another, in other respects. The most completely stated version of such an

alternate theory is found in various works of Theo Vennemann, which will be relied on here as a primary source of information about the naturalist view of phonology (Vennemann 1971, 1972a, 1972b).[2]

In certain respects, the naturalist tendency can be seen as a healthy reaction to some of the more extreme positions that emerged from the quasi-etymological approach. In many of these areas, Vennemann's natural generative theory is a definite improvement over the Hallean approach with which the quasi-etymological tendency is closely associated. Among the areas in which such improvements are made are: 1) the recognition of the importance of the syllable in phonological theory--a point almost completely ignored by Halle and his immediate followers; 2) a diminution of the complexities of rule ordering;[3] and 3) the apparent willingness of Vennemann to recognize something more closely approximating the classical phonemic level than anything allowed in Hallean theory. These factors seem to be strong enough, in fact, to make the naturalist view preferable in a forced choice between just these two alternatives.

It would certainly be most unwise, however, for a linguist to assume that phonological theory is limited to just these two or any two alternatives. The actual choice is not even limited to those theories which have been actually articulated, since it is possible to arrive at a new but nevertheless consistent and coherent synthesis by combining existing precepts in a novel ways, or even to come up with a wholly innovative alternative. In view of these possibilities, therefore, the prudent linguist will consider every aspect of each proposed theory, evaluating each of these aspects both individually and with regard to coherence of fit with other aspects of the theory. If no existing theory seems totally to his liking, he will further consider the possibility of new syntheses.[4]

In looking at Vennemann's proposal from this point of view, one may find that the most disturbing aspect of his theory is the severe restriction it imposes on the abstractness of morphophonemic (=underlying) shapes. One restriction proposed by Vennemann 1971 is as follows:

> The lexical representations of roots consist entirely of segments occurring in phonetic representation.

To this there is added a further restriction as follows:

> Lexical representations of roots are identical to one of the radical 'allomorphs' of the paradigm, plus an (often empty) set of suppletion rules.

Following Vennemann's own terminology, this second requirement will be termed the <u>strong naturalness condition</u>.

The strong naturalness condition, a peculiarity of Vennemann's particular proposal, wi be discussed first. The weaker principle will be accorded discussion in section 3.

The proposed strong naturalness condition seems to constitute a return to a type of morphophonemic analysis and description used by some of the Neo-Bloomfieldians. In their writings it was termed the 'basic allomorph' method of description (as in Nida 1949:45) or the 'base form' identified with one of the actual allomorphs in Gleason 1961:82. The key assumption which identifies this Neo-Bloomfieldian notion with Vennemann's proposal is the insistence that the underlying form be identical to one of the actually occurring allomorph In the treatment of a large number of cases of morphophonemic alternation, it turns out tha a workable description can be constructed without violating this condition. This fact may be responsible for an initial, though misguided, confidence in its general effectiveness. It turns out, however, that there are many other examples in which a very general type of alternation cannot be handled with the generality it deserves if such a strong restriction is insisted upon. This point will be concretely examined in connection with the Russian data presented in the table below, consisting of some partial noun paradigms from Contemporary Standard Russian. The suffixes in these forms are /a/, /e/,

Nominative Singular	galavá	baradá	slabadá	travá
Locative Singular	galav,é	barad,é	slabad,é	trav,é
Nominative Plural	gólavi	bóradi	slóbadi	trávi
Genitive Plural	galóf	barót	slabót	tráf
	'head'	'beard'	'settlement'	'grass'

Table: Partial Russian Noun Paradigms

/i/, Ø, respectively, leading the remainder of each word as constituting the stem (as underlined in the table).

These realized forms, cited here in classical phonemic transcription, exhibit alternations affecting (1) the position of stress, (2) vowel quality (for the first three items only), and (3) the voicing and palatalization characteristics of the stem-final consonant, which may be either voiceless plain, voiced plain, or voiced palatalized. Further examination shows that the stress and vowel quality alternations are interrelated, so that for the vowels of these stems a stressed vowel will be /o/, and an unstressed one will be /a/. The fourth partial paradigm has been included to illustrate the fact that some stems have invariable /a/, occurring without alternation with /o/ regardless of stress characteristics.

A full examination of Russian stress, which involves many more considerations than can be presented here, will show that stems and suffixes can be marked with certain stress characteristics which would allow the predictability of the correct stress in these various forms.[5] For the purpose of further discussion here, it will be assumed that stress can be predicted from a properly established morphophonemic representation of each of these Russian forms, and detailed discussion will be concentrated on the remaining alternations.

These remaining alternations turn out to be among the most pervasive in all of Russian phonology. They may be informally described in the following statements:

A. An underlying /o/ is realized as a phonemic /a/ when not stressed.

B. An underlying voiced obstruent is realized as the corresponding voiceless phoneme when final (and in certain other positions not pertinent here).

C. An underlying plain consonant is realized as its palatalized counterpart before /e/.[6]

A formalization of these three principles will completely account for our paradigms (apart from stress) provided that the underlying shapes of the roots are taken to be the following: golov, borod, slobod, trav. The critical point is that the vowels of the first three examples must all be o's in order to allow the automatic prediction of this alternation. By Vennemann's strong naturalness condition, however, this could not be done, because the surface forms of the root, all of which are exemplified by this partial paradigm can never contain two /o/'s as a direct consequence of the facts that (1) realized /o/ occurs only under stress, and (2) only one stress can occur per phonological word. As a result the strong naturalness condition leaves us unable to account for these forms as matters of regular morphophonemic alternation.

Apparently the only alternative that Vennemann and his followers would have in such an instance is a resort to suppletion. But since the alternations involved are widely recurrent, it seems extremely uneconomical to have to use suppletion in such

instances merely in order to avoid violating the strong naturalness condition. In the light of such data, which are likely to be found in other languages as well, the most reasonable course would seem to be the abandonment of this condition--or rather its rejection since presumably only a few linguists have even tentatively accepted it.

3. The phonetic projection principle.

It is the claim of this paper that both the quasi-etymological variety of generative phonology exemplified by the work of Lightner, Schane, and others, and the 'natural' generative phonology proposed as an alternative to it by Vennemann are unsatisfactory as characterizations of the phonological systems of natural human languages. The unacceptability of the first is seen most clearly in its overly strong claim about the ability of the native language learner to function as a very sophisticated and powerful internal reconstruction device--one capable not simply of matching up correspondences, but of actually constructing a phonetically plausible underlying form and set of derivational rules for the occurring alternatives. The 'natural' alternative, on the other hand, suffers from an underestimation of the learner's ability to a certain extent. It completely denies the ability to use underlying phonological representations which are not identical to some one of the occurring forms of a root.

In view of the fact that the inadequacies of these two views are matters of opposite extremes, one overestimating the learner's ability while the other underestimates it, it might seem that we could reasonably conclude that an adequate treatment could be achieved with the adoption of a version of generative phonology intermediate in the degree of abstraction which it allows. The title of this paper, however, makes the claim that the errors of quasi-etymological and 'natural' generative phonology are more closely related than they might seem to be at a cursory glance. While they are indeed at opposite poles along one dimension of contrast, they agree on another point which in fact lies at the root of their problems. This point of agreement is the acceptance of a principle which may appropriately be termed the phonetic projection principle. This principle assumes that underlying phonological forms (i.e. morphophonemic shapes) are made up of the same material--namely phonetic feature specifications--as the phonetic output of a phonological system. The use of the the phrase 'phonetic projection' in characterizing this principle comes from the fact that its use involves the projection of the substantive elements of phonetics into the form of language.

The loosest interpretation of this principle would simply require that underlying shapes of all languages be statable in terms of a universal set of phonetically specified features. This interpretation would not preclude the arbitrary use of features specifying such properties as aspiration or glottalization in languages which show no such phenomena on the surface.

A slight tightening of this requirement would require that the features in question be selected from among those actually found on the surface of the language in question. Postal's naturalness condition (Postal 1968:53-96) would seem to entail at least this degree of tightening.

A further restriction can be found in the first principle quoted from Vennemann, which requires not merely the selection of the features from among those occurring on the surface, but that of the individual segments as well. And with Vennemann's strong naturalness condition we in effect see the extension of this type of requirement to the rank of morphs.

As a common minimum, however, all these versions of generative phonology share the assumption of phonetic projection, the basic notion that the underlying and surface levels of phonology can be allowed to differ only in degree and not in kind. The general theoretical objection to this sort of principle is that it runs contrary to the

Saussurian-Hjelmslevian dictum that language is a form and not a substance by including substantively defined elements within the linguistic system, which is supposed to be a form. On a less theoretical level, we can see in both quasi-etymological and 'natural' phonology the consequences which such an assumption can lead to. One way to satisfy the requirements of phonetic projection is to adopt the quasi-etymological view and construct characterizations of the synchronic phonology of the language amounting to a recapitulation of its diachronic derivation. In true historical phonology, of course, some analog of phonetic projection is required, though the projection involved is within the temporal dimension rather than within the structural dimension. Another alternative is that offered by the naturalist: build further restrictions into phonological theory with the consequence that many quite regular alternations have to be treated as suppletion. It could, of course, be argued that the naturalists are fundamentally on the right track, but that it is only the particular proposal of the strong naturalness condition that is in error. To one who believes this, the solution would be to find a weaker form of naturalness which provides satisfactory treatment of the data without going to the quasi-etymological extreme. It is a major point of this paper, however, that the phonetic projection principle itself is the major source of the problem and to show that its abandonment will provide a viable alternative, as well as being more in line with the notion that language is form rather than substance.

The defense most usually offered for the use of phonetic features in the specification of underlying forms belies an inaccurate appreciation of the alternatives involved. It consists of a declaration that if underlying phonological forms are not constrained in any arbitrary material--the absurd example of 'palm trees' has sometimes been cited.[7] Since we don't want underlying forms to consist of palm trees which then somehow get replaced by phonetic features in the end, the argument goes, we obviously must constrain them so that they are expressed in terms of the same set of universal phonetic features as the phonetic output of the phonology. For purposes of discussion, this particular line of reasoning will be termed the 'palm tree argument'. It is, in fact, a fallacious argument because it considers only the ridiculour alternative and ignores the more profound one. The more serious alternative which it ignores is that underlying representations do not contain any sort of substative elements at all, but are wholly defined by their relationships to the morphemes on the one hand and their ultimate phonetic realizations on the other. This alternative has been maintained in stratificational theory for close to ten years.[8] It is in line with the general assumption of stratificational theory that the Saussure-Hjelmslev dictum that language is form rather than substance should be taken seriously. Those who take phonetic projection for granted and defend it by taking refuge in the palm trees, however, have either overlooked or failed to understand this stratificational alternative.

The phonetic projection principle is actually a consequence of a still more fundamental assumption, which is today probably the only common trait of all multitudinous varieties of transformational-generative theory that have cropped up over the past decade. It may be termed the <u>derivation axiom</u>, and it consists in the assumption that the relationship between linguistic levels is a matter of deriving the shallower or less abstract from the deeper or more abstract.[9]

Sydney Lamb has repeatedly attacked the assumption incorporated in this axiom, though with limited success if we judge from the number of adherents it still retains. Lamb has sometimes compared this sort of assumption in linguistics with the assumption that the relationship between man and the apes can be accounted for only by assuming them both to be descended from a common ancestor. The analogy with the synchronic linguistic situation is not entirely precise, since even in the more sophisticated alternative genetic descent or derivation is still involved. But the basic parallel still exists in that the more sophisticated alternative in both cases involves the recognition of an entity which does not appear on the 'surface'--the common ancestor in the biological case, the morphophoneme or other linguistic abstraction in the synchronic linguistic case. Lamb has further argued that the phonetic projection principle is a direct

consequence (Lamb 1967) of the derivation axiom. If one assumes that underlying forms are objects from which surface substantive objects are derived by a series of processes, one will reason that only some will need to get changed in a given derivation and will be led by considerations of economy quite general to science to adopt the phonetic projection principle rather than provide rules to change each and every specification.[10]

So a strong argument in favor of the phonetic projection principle can be devised only if the derivation axiom is assumed. If one abandons the derivation axiom and considers the possibility that underlying forms are relational rather than substantive, one is never led to anything like the palm tree argument. But if the derivation axiom is assumed without question, as has widely been done,it appears that the only alternative to adopting phonetic projection is to allow phonetic specifications to be derived from palm trees or whatever other fanciful objects one chooses to use.[11]

Even rejecting the derivation axiom, however, some may want to insist that some version of the phonetic projection principle is valuable as a constraint upon the arbitrariness of phonological theorizing. In a properly constructed theory of linguistic form, however, such substantive constraints are in fact unnecessary. They are unnecessary because such a theory will have to require the form of language to be anchored to substantive reality at both the content and expression ends of the system. It is only <u>within</u> the form that there are no substantive elements. And even the arbitrariness of this form will be further constrained by a requirement of simplicity, which demands that the form connect the content substance with the expression substance in as simple a manner as possible congruent with the consistency and exhaustiveness of the accounting it provides. Expressing ourselves in terms of Hjelmslev's glossematics (Hjelmslev 1961) we can say that the linguistic system is <u>arbitrary</u> in the sense that it does not contain elements of linguistic substance, but at the same time it must be anchored to substance at both ends in order to satisfy the concommitant requirement of <u>appropriateness</u> to the description of language, and the further Hjelmslevian requirement of simplicity will serve to further restrict arbitrariness. Since the requirements of appropriateness and simplicity do restrict the seeming arbitrariness of linguistic theorizing in this manner, it is not necessary to resort to such extraneous considerations as the phonetic projection principle, which inevitably obliterate the distinction between form and substance.

It therefore appears that the phonetic projection principle can be made to seem justifiable only within the context of the derivation axiom. This axiom, furthermore, is the most fundamental and pervasive characteristic of transformational linguistics in all its forms,[12] and is also its most fundamental error. It has been assumed without question or proof, perhaps because its adherents could see only pure taxonomy--the classification of the data--as an alternative to its adoption. But in fact its adoption has led to systems which fail to deal with the form of language as opposed to the substantive data itself in the same sense that pure taxonomies fail to do so: they fail to depart from the substantive elements.

A more viable alternative to pure taxonomy than that offered by the derivation axiom has been available to linguists for a long time. Its foundation may be expressed in the form of a <u>relational axiom</u>: the assumption that language is a complex integrated network of relationships, connected to the substance of both expression and content but at the same time distinct form either. This fundamental assumption lies at the foundation of the tradition going back to Saussure, Firth, and Hjelmslev, which has Lamb and Halliday as its most illustrious contemporary followers.

The reasons for preferring the relational axiom over the derivation axiom rest squarely in the area of realism. The relational axiom has a vastly better chance of being congruent to the nature of the human brain than does the derivational alternative. The brain is known to contain a vast interconnecting network of neurons capable of communicating with each other through a few kinds of electrical signals, and connecting to both the various sensory organs and the various motor functions of the body. Language is plausibly conceived of as residing in the part of this total network which connects

ideas[13] to articulatory and auditory activity. So even though the proposals of different followers of the relational approach may fall short of complete congurence with the workings of the brain, the general approach of this group at least has the advantage of being compatible with a plausible theory relating to the nature of the brain.

Followers of the derivation axiom have generally shied away from proposing anything so specific as a working hypothesis about how the knowledge intended to be captured by their theories can be stored in and used by the brain. Chomsky 1972, for example, discusses the actual relationship between language and mind only in very abstract terms. Derivationalists have generally simply assumed that some correlation of their theory of linguistic knowledge must exist in the brain without ever suggesting how this could be so.

One derivationalist who has gone beyond this vagueness to at least a small degree is Wallace Chafe, who is strongly committed to the derivation axiom despite his considerable disagreement with orthodox transformationalism on many points. In Chafe 1970, he speculates as follows (p. 75)

> Concepts...are located deep within the human nervous system. Presumably they have some physical, electro-chemical reality there, but as yet we are unable to make any direct use of it for linguistic purposes.

This view seems to suggest that the concepts are stored in the brain in some sort of electro-chemical way, and that the electro-chemical signals in question undergo various changes correlating with his various mutation rules.[14]

One view congruent with the relational axiom, on the other hand, suggests that concepts are stored in the connectitity of the neural network, relating memories and sensations through language to speech outputs by arbitrarily learned conventions. This view thus envisions a much clearer role for the connections in the neural network than the purely electro-chemical view suggested by Chafe and more compatible with the derivational view.[15] In view of this fact, the relational hypothesis seems to be preferable in the absence of proof of its untenability or the availibility of a still more plausible alternative which could supercede it.

4. Panchrony: Progress or confusion

A further weakness of the derivational approach results from its use of the same terminological and notational conventions for both synchronic relations and diachronic change. Such a usage creates the danger that the repeated use of identical conventions in these two distinct areas may cause their users to fail to make the very real and necessary distinctions between synchronic and diachronic phenomena. It is fairly obvious that it may be a particular problem for beginning students to keep the necessary distinctions clear in view of these identities, but there is also some evidence that more mature scholars, who should know better, have been similarly misled.

Chafe, for example, seems to believe that synchronic morphophonemic rules as well as certain rules dealing with idiomaticity should recapitulate the historical changes which have led to them. Toward the end of Chafe 1970, for example, he denies the reality of his process approach as applied to the formation of semantic structures, but at the same time reaffirms it with regard to the so-called postsemantic processes, and he does this on the basis that the postsemantic processes correspond to historical changes, while the formation rules do not. He seems to take it as self-evident that historical considerations justify the use of process in a synchronic account in the post-semantic cases (pp. 350-1). The same sort of confusion seems to be evident in his criticism of stratificational theory on the grounds that 'history is not stratified' (Chafe 1968:598).

This criticism is in no way intended to deny the value of recent studies interrelating synchronic alternation and dialect variation with language acquisiton and historical change [many growing out of the seminal article by Weinreich, Labov, and Herzog (1968)]. Panchronic studies, in the sense of studies of such interrelationships, are providing important and illuminating clarifications of both linguistic structure and linguistic change. But panchronic studies without a fundamental recognition of the different nature of the types of phenomena involved, despite their interrelationships, will be a confusion rather than an illumination.

Current linguistics, with its widespread and uncritical acceptance of the derivation axiom, is finding it difficult to avoid this fatal pitfall. Operating with the relational axiom, on the other hand, one is forced from the beginning to make a distinction between a change on the one hand and an alternative in the system of a speaker at a given time. The notation and terminology of a relational system simply would not lend themselves to such a confusion. These distinctions do not prevent the study of interrelationships between change, dialect variation, and alternation, but rather provide a sound framework for progress in this area without confusion.

In short, panchronic studies will lead to progress only if they are pursued in a framework which recognizes the essential distinctions between the phenomena involved in synchrony and diachrony, despite their interrelationships. If they are pursued in the unrealistic framework provided by the derivation axiom, however, there is great danger that such studies may lead as much to confusion as to enlightenment. Such a danger provides still another reason for the reconsideration and rejection of the derivation axiom in linguistics.

FOOTNOTES

1. Basically, a strong position was one in a stressed syllable or in a syllable before one with a weak jer, while all others were weak. In some languages, including Russian, a jer next to a liquid was also given special treatment.

2. David Stampe uses the term 'natural phonology' for a theory having both similarities and differences from the views of Vennemann. Aspects of this view are discussed in Miller 1972, 1973, Nessly 1973, Rhodes 1972, 1973, Stampe 1972. Aspects of naturalism are also abundant in the recent works of other generative phonologists such as Schane (1972) and others.

3. The derivation-based nature of Vennemann's view of language structure, however, does not allow as complete an elimination of rule ordering as would be possible under alternate assumptions. See Lamb 1972 for a further discussion. Also cf. section 3 of this paper for discussion of the 'derivation axiom.'

4. The overly-limited choice of item-and-arrangement vs. item-and-process theories is an example in which linguistic discussion has concentrated excessively on an overly narrow set of alternatives. Altogether too much of the discussion of the treatment of morphological phenomena in the last 15 years has assumed that IA and IP are essentially the only alternatives open, and have taken demonstrations of the inadequacies of IA to be arguments in favor of IP. Stratificational theory has meanwhile achieved a synthesis which combines the advantages of both these approaches in a new alternative which cannot be characterized as either IA or IP (in view of the non-recognition of 'items' as essential parts of linguistic structure).

5. A stratificational formalization of a stress system originally worked out by Bulcsu Laszlo, though never published by him, is presented in Sullivan 1969.

6. It may ultimately be preferable to consider the palatalization in question to be part of the locative case suffix rather than a part of the stem. This question is ignored in the main text since it is irrevelant to the point under discussion.

7. Perhaps the original reference is to the use of arbitrary symbols such as &, ß, or ✱ in morphophonemic transcriptions, since the latter of these could, because of its shape readily be termed a 'palm tree'.

8. This alternative was first suggested in print in Lamb 1966a (a paper originally delivered in 1963). By 1965 Lamb had come to favor this alternative, as reflected extensively in Lamb 1966b and his later writings.

9. As pointed out by W. J. Sullivan in discussion following the oral presentation of this paper, the assumption of the derivation axiom is not actually consistent with the notion of a separate level in the first placé. This recognition of the existence of the levels while continuing to relate them by means of derivational processes has been termed the 'quasi-stratificational degree of sophistication' by Lamb 1966b, 1967.

10. The essentials of the argument are presented in Lamb's review of Chomsky's Current Issues and Aspects (Lamb 1967). Postal 1968:198-207 and Palmer 1968:294 exemplify the fallacious assumption that the separation of stratal levels requires uneconomical descriptions.

11. The derivation axiom is further responsible for the recent pseudo-argument among different transformationalists as to whether adjectives should be considered nouns or verbs in the deep structure. This results from a higher-level analog of phonetic

projection in which the categories of surface syntax (such as noun and verb) are projected into the deeper level, in preference to the establishment of wholly separate categories on the basis of the proper structure of this level of abstraction.

12. The term macrotransformationalism is a useful designation for the various schools and sub-schools which, following Chomsky, assume the need for transformational processes in grammar and phonology.

13. Ideas are themselves relational in nature, though we can treat them as items for convenience when our principal interest is in language proper.

14. This interpretation is perhaps more than Chafe intended by this remark. In any case, it should be pointed out that Chafe's work since 1970 has resulted in significant observations concerning the relation of language to psychology less colored by such assumptions about the nature of linguistic structure as the quoted statement implies (cf. Chafe 1972, 1973, 1974).

15. The view that knowledge is stored in the connectivity of the neural network should not be taken as implying that electro-chemical properties are completely unrelated to such storage. It does not imply that RNA (ribonucleic acid) does not play a role in memory, although far too little about this chemical and its function is known to make definitive conclusions. In view of the widely-used distinction between short-term and long-term memory, for example (cf. Chafe 1973 for further elaboration), it might be worth while to investigate the notion that short-term memory is stored in RNA while long-term memory (and linguistic knowledge) are stored in neural connectivity. RNA may also play an important role in the conversion of short-term memory to long-term when this process occurs.

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LATINATE ENGLISH VERBS AND NOUNS: A SYNCHRONIC, DIACHRONIC, AND PANCHRONIC DESCRIPTION

Doris H. Welch
University of Chicago and University of Illinois
at Chicago Circle

There is a relatively small set of linguistic forms in English that occur in approximately 14,000 common lexemes in the language and close to 100 ,000 lexemes, or one tenth of the entries in the unabridged Oxford English Dictionary.[1] This set is comprised of a mere twenty prefixes and fourteen roots, historically related to either the Latin or the Greek language. (See Appendix A) Words such as uncomplicated, reduce, production, aspect, and dispose consist of representative members of this set.

To linguists these learned words have presented a dilemma. Hockett states the problem: 'English has many words of the type remote, demote, promote, reduce, deduce, produce, each apparently built of two smaller parts, a prefix re-, de-, pro-, or the like, and a second part -mote, -duce, or the like. But the relationships of meaning are tenuous. Grammarians are not in agreement. Some brush aside the semantic difficulties and take each word as two morphemes, following the phonemic shapes; others regard the parallelisms of phonemic shape as unconvincing and take each word as a single morpheme.' (Hockett 1958:173)

Evidence of this dilemma may be seen in the various approaches taken by linguists in the analysis of this lexical class. In his descriptive statement, Bloomfield follows the principle of immediate constituents which, he says, 'leads us, at the outset, to distinguish certain classes of words.' (1933:209) Accordingly, words comprising forms of this particular set, i.e. re-ceive, de-ceive, con-ceive, re-tain, de-tain, con-tain, are classed as derived primary words, containing more than one bound form. Hence, Bloomfield recognizes the prefixes re-, de-, con-, and the roots -ceive and -tain as bound morphs.

Joseph Greenberg (1960:188) applies the minimal square test in his approach to this problem. 'We start with a set of forms that will be hereafter called a square. A square exists when there are four meaningful sequences in a languagewhich take the form AC, BC, AD, BD . . . . Where a square exists with corresponding variation of meaning, we are justified in segmenting each of the sequences of which it is composed.' To illustrate, the set eating (AC) : sleeping (BC) : : eats (AD) : sleeps (BD) corresponds to meaning variation and therefore may be segmented into eat-, sleep-,-ing, and -s.

The sets deceive : receive : : deception : reception : : deceit : recei(p)t are not as easily segmented because the segments themselves lack meaning. Greenberg refers to squares of this type as semantically defective. Under this condition, 'if there are parallel non-automatic

variations, the analysis is permitted even though definite meanings cannot be assigned to the morphs.' Thus, the above set justifies the segmentations de + ceive and re + ceive. (Greenberg 1960:189)

Both Bloomfield and Greenberg segment the forms, but Hockett seems more concerned with the status rather than the constituents of the forms. 'An obvious practical step is to set the morphemic problem aside, recognizing that each form is an idiom whether it is one or more morphemes.' (Hockett 195:173)

Fred W. Householder, Jr. in his theory of linguistic primes suggests that these segments (e.g., pro-, de-, in-, -late, -fer, -duce, etc.) 'cannot in general be primes of the sentence-grammar, or idioms, (but) they can perfectly well be primes of the idiom-grammar, etymemes or morphemes in this specific sense.' (1959:237)

All of these linguists agree that the forms may be segmented. The dilemma stems from the fact that the forms are semantically deprived, i.e. they carry no meaning of their own. In Idiom Structure in English, Adam Makkai, following Conklin (1962) and Lamb (1966), calls these forms lexemes, 'the nature of every lexeme being that its meaning cannot be predicted from its morphological constituents.' (1972:117) To illustrate, one cannot predict the meaning of conduct by examining con + duct. As previously mentioned, these are empty categories and are not, for the average native speaker, semantically sponsored. However, stratificationally, we can consider them elementary units on the lexemic stratum, or technically, lexons whose realizations are morphemes in this specific sense. (1969:12)

On Lamb's relational network diagram, these units are poly-lexonic lexemes, and because stratificational grammar is a system of relationships, these forms are identified not only as to structure but also as to their relationships to other forms. Thus, as seen in Figure I, the unit conduct is one lexeme, a realization of a semon on the sememic stratum; it is also two morphemes, realizations of the lexons Ln/con-/ and Ln/duct/ on the lexemic stratum. As such, the lexemes are sememically sponsored, the lexons are lexemically sponsored. Since the meaning of a lexeme is its sememe, the criterion of meaning-bearer has been lifted from these controversial segments and relegated to the sememic and hypersememic strata. Hence, within the framework of stratificational grammar, this dilemma promises to be resolvable.

However, describing the linguistic status of this small set of Latin and Greek elements (lexons) on a relational network diagram is merely the preliminary step in this analysis. The main concern of this paper is to describe the synchronic and diachronic phenomena of these forms on a relational network, and to propose a panchronic treatment for the purpose of consolidating and simplifying the data.

The method used will simulate that of a native informant in the elicitation process. The elicitations of two hypothetical informants, (1) a native speaker of English and (2) a native speaker of English who is also a Latin scholar, will illustrate a synchronic description and a pseudo-diachronic description, respectively.

A Synchronic Description

A synchronic description of learned words based on the principles of the relational network system and the elicitations of a hypothetical native informant of English is illustrated in Figure II. The description, of course, is partial treating only four lexemes: L/attend/, L/accept/, L/admit/, and L/aspect/.

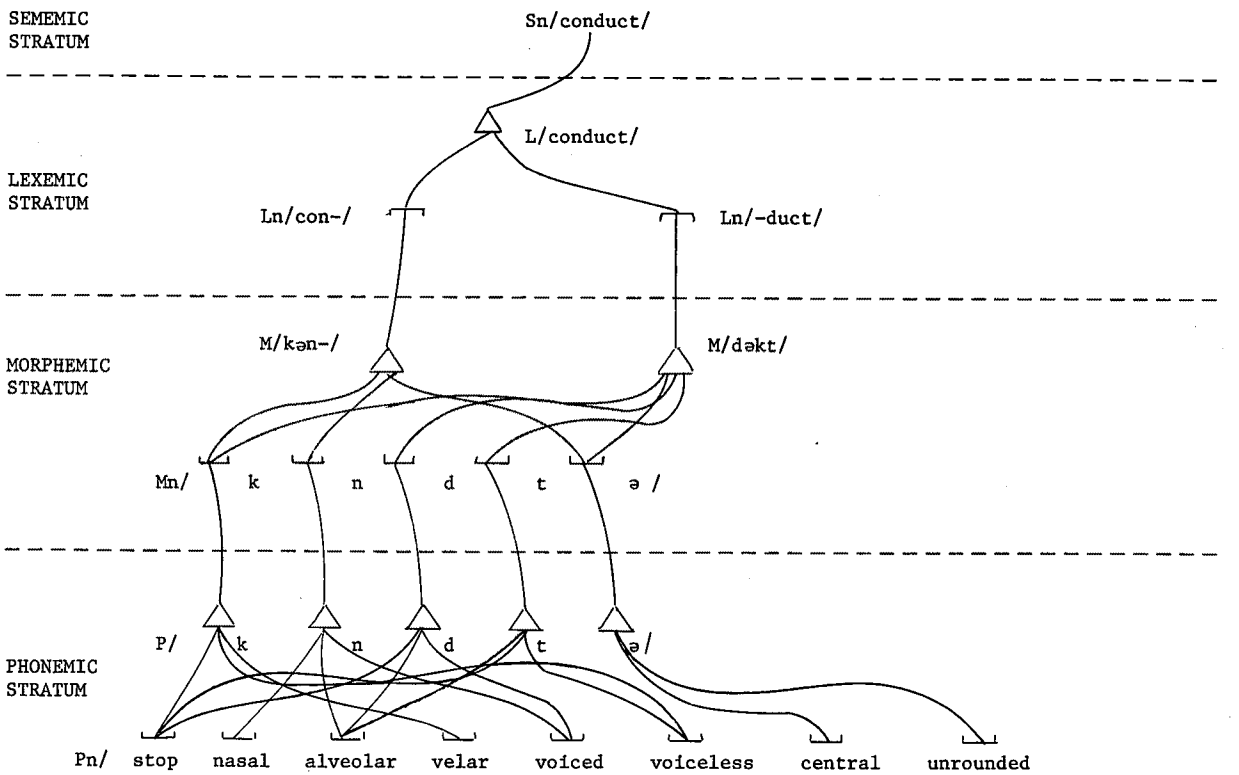

Figure I

Relational Network Diagram of L/conduct/

The process of linguistic analysis as viewed on the relational network would place the phonetic transcription on the hypo-phonemic level with the phonemic and morphemic analysis being represented by their respective strata. The upper lexemic stratum is the neutral territory for both informant and linguist. It is this point in the network that marks the limitations for both participants. The linguist cannot go above this level without the assistance of the informant in glossing the lexical forms. Similarly, the informant cannot go below this level because his knowledge of the language in reference to these strata is subliminal. Therefore, the informant's level of awareness of his language may be said to originate in the lexeme. The lexon is beyond his awareness since most native speakers do not consciously recognize such sub-lexemic forms as a-, ad-, ac-, -tend, -mit, -cept, and the like. The reason is that, for the informant, these forms have no referents in the real world. Hence, the linguist cannot consider the morphemes M/ə/, /æ/, /æk/, or /æd/ alternations of a particular lexon. (See Figure II) To set up a hypothetical base form (e.g. Ln/ad-/) and describe the above forms as alternations would be assuming that the Ln/ad-/ had sememic sponsorship.

It is entirely possible, however, that the native speaker may be aware of the recurrence of these forms in his language. There may be occasions when one form triggers an association with a similar form (e.g., the -cept of concept may be associated with that of precept or accept.)

Pseudo-Diachronic Description

In contrast to the synchronic description of the polylexonic lexemes, or post-idioms of Makkai (1969:13), a pseudo-diachronic[2] description of this same class of words is illustrated in Figure III. Because the hypothetical informant in this instance is a native speaker of English and also a Latin scholar, the description is considered pseudo-diachronic.

A comparison of Figures II and III reveals several predominant differences in the networks. Informant A (Figure II) has within his sememic system certain semons which are symbolized by four English lexemes. His knowledge behind these lexemes is only in relationship to lexeme and semon. Informant B (Figure III), on the other hand, shares the same knowledge as Informant A (a fictitious situation), but in addition he has cultural information that supplements what he knows about these particular lexemes. He is capable of viewing these forms as words or as parts of words, all of which have sememic sponsorship. Technically, Informant B has in his linguistic network two structural classes of lexemes: bi-lexonic lexemes or morphotactically free words, such as attend, and mono-lexonic lexemes or nonword elements, such as tend. The forms which are lexons for Informant A have lexemic status for Informant B because he is able to attach meaning to these symbols (e.g. tend comes from the Latin verb tendere meaning 'to stretch'; cept from capere 'to take or seize'; mit from mittere 'to send'; spect from spectare 'to see'; and ad-, which is the Latin preposition meaning 'at, to, or toward').

Furthermore, in Informant B's sememic stratum the linguistic forms not only have meaning based on experience, i.e. experience of agreeing, receiving, ministering to, etc., but also meaning based on formal knowledge, i.e. accumulation of abstract facts, such as the Latin form capere means 'to take or seize'. The two categories of meaning are realized by different types of lexemes, i.e. bi-lexonic lexemes or mono-lexonic lexemes. Consequently, in Figure III, the morphemes /æk/, /æd/, /æ/, and /ə/ may be viewed as alternants of the hypothetical base

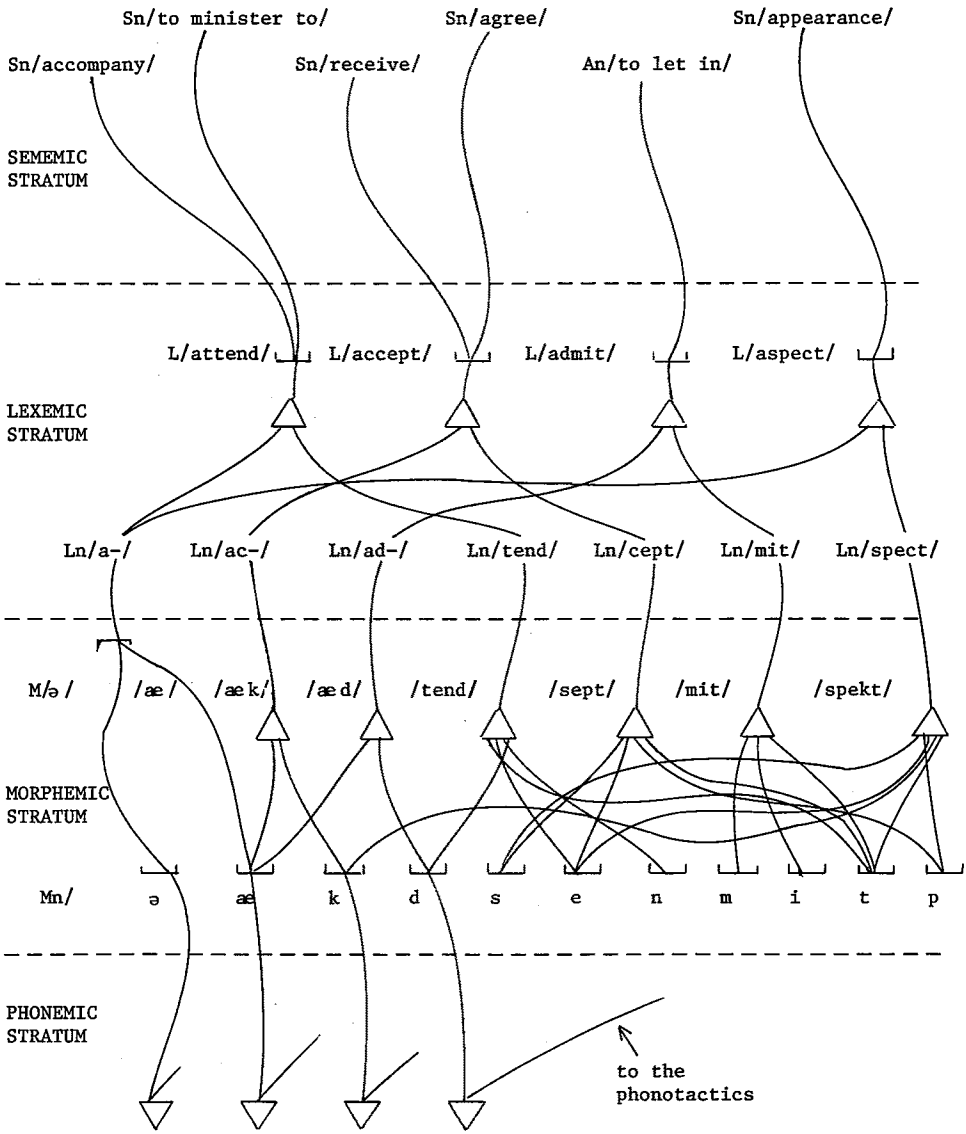

FIGURE II[7]

A Partial Relational Network Diagram of Four Lexemes in the Vocabulary of an Average Native Informant of English

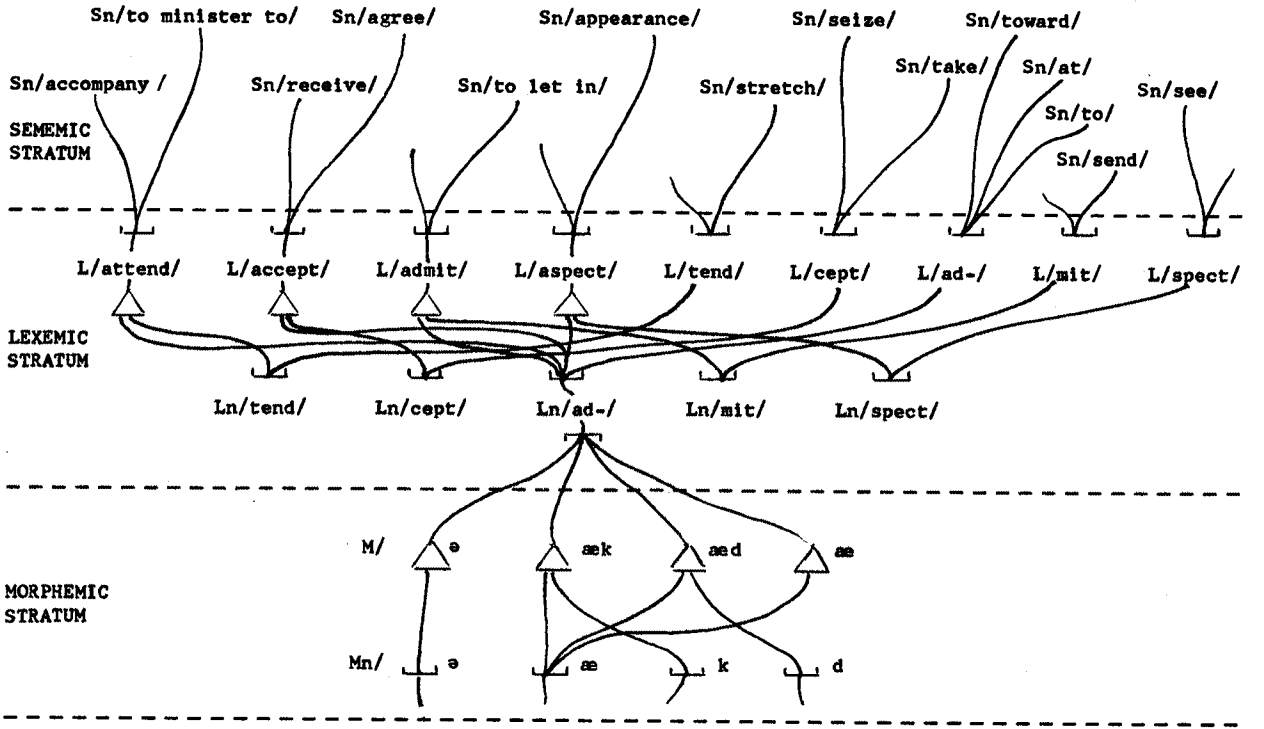

FIGURE III

A Partial Relational Network Diagram of Two Classes of Lexemes in the Cognition of a Native Informant of English Who is Also a Latin Scholar

Ln/ad-/ which has lexemic sponsorship and therefore sememic sponsorship. In Figure II, these morphemes are related to the lexemic level, but only as separate lexons, not as alternants of a base. The hypothetical base Ln/ad-/ is by-passed by the native speaker (Informant A), who has no knowledge of the underlying form.

Further comparison reveals that the extent of Informant B's language awareness goes beyond that of Informant A. His awareness of Latin prefixes and roots (lexemic stratum), his knowledge of the alternations of these forms (morphemic stratum), and his knowledge of the reasons for these alternations (phonotactics) represent his linguistic network.

Panchronic Description

There used to exist in the discipline of linguistics an unwritten law which would disallow the union of diachrony and synchrony. Ferdinand de Saussure in his Course in General Linguistics (1959:98-99) did not equivocate when he stated: 'Whichever way we look in studying a language, we must put each fact in its own class and not confuse the two methods' (i.e., synchronic and diachronic linguistics). This dictum was taken at face value and exaggerated during the Neo-Bloomfieldian structuralist period, especially in the U. S. A. This unwritten law or principle of language analysis is, of course, well-suited to the language which has never been subjected to linguistic analysis, and that precisely was the main endeavor of the Boaz-Sapir-Bloomfield line of linguistics.

But for a rich literary language such as English, which possesses an extremely complex orthography and lexicon plus a written history of many centuries, it would seem entirely appropriate to modify this unwritten law under optimal modern circumstances. If one is concerned with a language-state, then the analysis should be synchronic. Likewise, if one is concerned with evolutionary facts of a language, the analysis should be diachronic. However, there are phenomena in complex, highly developed languageswith a word-stock of more than one family which occur with great regularity but are inexplicable, or partly so, because of the barrier erected between synchrony and diachrony. Thus, in the tradition of Bernard Bloch, who used both synchronic and diachronic facts in his analysis of the English verbs show-ed and show-n, if one is concerned with the analysis of a current language-state but encounters linguistic forms within that language-state which can be described further by examining a different state of that language, a panchronic method is suggested.

De Saussure defines synchronic and diachronic linguistics as follows: 'Synchronic linguistics will be concerned with the logical and psychological relations that bind together coexisting terms and form a system in the collective mind of speakers. Diachronic linguistics, on the contrary, will study relations that bind together successive terms not perceived by the collective mind but substituted for each other without forming a system.' (1959:99-100) Panchronic linguistics, according to my definition, will have its basis in synchronic linguistics but will draw on the resources of diachronic linguistics to assist in the description of recurring linguistic forms which partially or totally resist synchronic analysis.

In the introduction to this paper, reference was made to a 'small set of linguistic forms' in the English language which constantly recur. A synchronic analysis of these forms on a relational network has determined their linguistic status, i.e. lexons, elementary units on the lexemic stratum; a pseudo-diachronic analysis has assigned meanings to

these forms and described their linguistic status as lexemes. Thus, an analysis of synchronic and diachronic information on a relational network diagram seems to partially solve the problem of describing these forms. But to simplify the description, a panchronic approach is needed.

As the definition suggests, a panchronic analysis will 'assist in the description' of these particular forms. In this case, the assistance is needed on the lexemic stratum. The synchronic analysis has isolated the forms (e.g. a-, ac-, ad-, -cept, -mit, etc.), but the analysis is still too cluttered. Thus, the diachronic analysis is appealed to. The meaning of these forms drawn from this analysis permits the formulation of a hypothetical basic lexon which accounts for variations in the alternation pattern.[3] Such a system reduces the lexonic inventory from several forms to only one as seen in Figure IV. Thus, a panchronic analysis may be used to reduce the inventory of linguistic forms and simplify the description.

An Applied Panchronic Description

Figure IV is a panchronic description of the hypothetical basic Ln/mitt/ on a relational network diagram. The description is partial and is a representation of diachronic and synchronic knowledge, not diachronic change. A single network is inherently incapable of representing change as such.[4] Preliminary synchronic analysis, i.e. the recurrence of similar non word elements, revealed that diachronic information was needed at the lexemic stratum. Investigation of the etymology of the linguistic form /mit/ disclosed that the form was historically related to the Latin verb mittere which has four principle parts: present indicative, mittō; present infinitive, mittere; perfect indicative, misī; and perfect passive participle, missum.[5] Removal of the verb endings gave the following roots, or morphemic signs: MS/mit/ and MS/mis/. This accounted for the two forms found in English words such as commit and missile. Further investigation revealed that the form mess as in message had originated in Latin but had passed through the Old French before coming into the English language.[6] Thus a third form, MS/mes/, was accounted for. The final form MS/mis/ occurs only in words with the suffix -ion. (See morphotactics - Figure IV) In Latin, -io was used to form nouns from the past participle of the verb (e.g. Latin missiō, pronounced /missiyō/). The English form -ion is historically related to the Latin -io, but in the derivational process involving this suffix, the final consonant of the root is palatalized to the /š/ sound. With this information, the morphemes $M/miss_1/$, $M/miss_2/$, M/mitt/, and M/mess/ may be designated as alternations of the hypothetical Ln/mitt/.

The efficiency of this procedure is illustrated by listing the hypothetical lexons and morpheme alternants for fourteen common roots in English. (See Appendix B) Instead of the lexonic inventory being seventy-three elementary forms on the lexemic stratum, there are now only fourteen lexons.[8]

The generative ability of one member in this small set of lexons is illustrated in the morpho-tactic pattern of Figure IV. Only one root, the Ln/mitt/, combining with eleven prefixes and eleven suffixes is capable of generating sixty-two lexemes.[9] If one root has this combinative power, it is not difficult to accept the fact that thirty-four such forms occur in approximately 14,000 common English words.

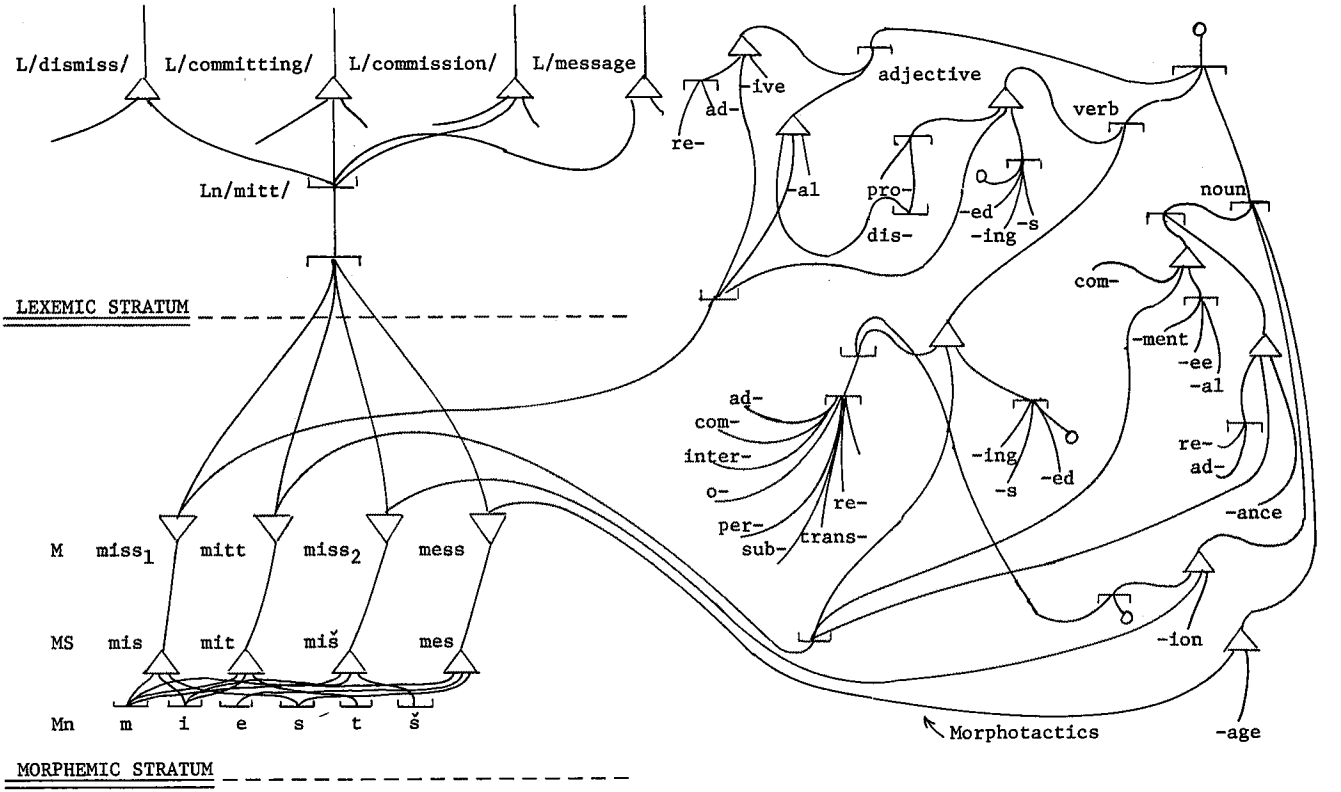

FIGURE IV
A Panchronic Description of the Hypothetical Basic Ln/mitt/
on a Relational Network Diagram

The generative ability of these particular roots is also significant in reference to the structure of lexemes that are derived. Polylexonic lexemes may be generated which consist of prefixes, bases, derivational contrasts, and inflectional endings. Lexemes may have as many as two prefixes but usually not more than four suffixes.

Polylexonic Lexeme	Prefix(es)	Base	Derivational Contrasts	Inflectional Endings
perfect	per-	fect		
perfection	per-	fect	-ion	
committed	com-	mitt		-ed
persistently	per-	sist	-ent -ly	
accidentally	ac-	cid	-ent -al -ly	
conventional-ization	con-	vent	-ion -al -iz -ation	
decompose	de- com-	pose		
imperfections	im- per-	fect	-ion	-s
unintentional	un- in-	tent	-ion -al	
unintentional-ly	un- in-	tent	-ion -al -ly	

This entire study may be summarized in the words of Sydney M. Lamb: 'Linguistic analysis can perhaps best be understood as a process of simplifying. It is a process in which the analyst finds recurrences of similar entities having such relationships among themselves and with other entities that they can be regarded, at some level, as repetitions of one another. Once found, such recurrent entities may be extracted and described only once instead of repeatedly. This process amounts to a simplification, and it also involves generalization. The phenomena dealt with are then better understood and are better treated in whatever applications may be desired.' (1966:3) The phenomena of Bloomfield's derived primary words, Hockett's idioms, Householder's primes of the idiom-grammar, Makkai's post-idioms are simplified on the lexemic stratum of a relational network system.

Footnotes

[1]cf. Brown, p. 4.

[2]The term pseudo-diachronic in my usage refers to the indirect method of a diachronic study, in this case the use of a hypothetical informant.

[3]This artificial base form is not an entity. According to Lamb, 'it is nothing more than a position in a network of relationships.' (1969:14)

[4]I am grateful to David Lockwood for bringing this to my attention.

[5]cf. Bennett

[6]cf. Webster's for etymological facts relating to data in this paper.

[7] I am indebted to Sidney Lamb for suggestions regarding the sememic stratum.

[8]These fourteen lexons are the hypothetical forms on the panchronic network.

[9]The total generative power of this root is not given here because the network would be too complex. Only a small sampling of prefixes and suffixes is used. As pointed out to me by Peter A. Reich, a feature of the tactic pattern is its productivity.

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Appendix A

The Fourteen Words
Keys to the meanings of over 14,000 words

Words	Prefix	Common Meaning	Root	Common Meaning
1. precept	pre-	(before)	capere	(take, seize)
2. detain	de-	(away, down)	tenere	(hold, have)
3. intermittent	inter-	(between, among)	mittere	(send)
4. offer	ob-	(against)	ferre	(bear, carry)
5. insist	in-	(into)	stare	(stand)
6. monograph	mono-	(alone, one)	graphein	(write)
7. epilogue	epi-	(upon)	logos	(speech, study of)
8. aspect	ad-	(to, towards)	spectare	(see)
9. uncomplicated	un-	(not)	plicare	(fold)
	com-	(together, with)		
10. nonextended	non-	(not)	tendere	(stretch)
	ex-	(out, beyond)		
11. reproduction	re-	(back, again)	ducere	(lead)
	pro-	(forward, for)		
12. indisposed	in-	(not)	ponere	(put, place)
	dis-	(apart, not)		
13. oversufficient	over-	(above)	facere	(make, do)
	sub-	(under)		
14. mistranscribe	mis-	(wrong)	scribere	(write)
	trans-	(across, beyond)		

Appendix B

Hypothetical Lexons	Meaning	Morpheme Alternants (based on spelling)	Examples
1. cap	to take, seize	cap	capable
		capt	capture
		cept	except
		ceipt	receipt
		ceit	deceit
		ceive	receive
		cip	municipal
		cup	occupation

Hypothetical Lexons	Meaning	Morpheme Alternants (based on spelling)	Examples
2. ten	to hold	ten	lieutenant
		tent	detention
		tene	tenement
		tain	retain
		tin	pertinent
		tinu(e)	continued
3. mitt	to send	mitt	committee
		mit	omit
		miss	missionary
		mis(e)	promise
		mess	message
4. fer	to bear, carry or bring	fer	conference
		fert	fertile
		lat(e)	relate
5. sta	to stand	sta	obstacle
		stat	stationary
		stant	constant
		st	stance
		stin	obstinate
		stit	superstition
		stitu	constituent
		stitut(e)	institution
		sist	assistance
6. spect	to look, see	spect	aspect
		spic	despicable
		spise	despise
7. plic	to fold	plic(e)	complicated
		pli	pliable
		ple	supple
		ply	imply
		plex	perplexity
		pleat	pleat
		play	displays
		ploy	employee
8. tend	to stretch	tend	attendance
		tent	contention
		tens(e)	extension
9. duc	to lead	duc(e)	induce
		duct	conduct
		duch	duchess
		duke	dukedom
		duit	conduit
		due	subdue
10. pon	to put or place	pon(e)	postpone
		posit	deposit
		post	posture
		pos(e)	deposed
		pound	propound
11. fac	to make or do	fac(e)	deface
		fact	satisfactorily
		fic(e)	suffice
		fect	affect

Hypothetical Lexons	Meaning	Morpheme Alternants	Examples
		fash	fashionable
		feat	feature
		fit	benefited
		feit	counterfeit
		feas	feasible
12. scrib	to write	scrib(e)	inscribe
		script	descriptive
13. graph	to write	graph	lexicographer
		gram	grammar
14. log	speech or word	logo	logogriph
		log	logic
		logue	catalogue
		logy	analogy

Appendix C

Lexemes Generated by the Tactics in Figure IV

admit	admitted	admitting	admits
admission	admittance	admissive	commit
committed	committing	commits	commission
commitment	committee	committal	missile
remissive	admissive	intermit	intermitted
intermitting	intermits	intermission	omit
omitted	omitting	omits	omission
permit	permitted	permitting	permits
permission	submit	submitted	submitting
submits	submission	emit	emitted
emitting	emits	emission	transmit
transmitted	transmitting	transmits	transmission
remit	remitted	remitting	remits
remission	promise	promised	promising
promises	dismiss	dismissed	dismissing
dismissed			

Alternation, Transformation, Realization, and Stratification Revisited

William J. Sullivan
University of Florida

0. Introduction

There are several issues in contemporary linguistics which might be called types of linguistic dichotomies, e.g. the directionality of language, inductive vs. deductive generalizations, process vs. arrangement. All of these issues had been decided conclusively twenty years ago. The issues were raised again and the accepted orthodoxies challenged by Chomsky, effectively beginning 17 years ago. Chomsky had many good points, and his arguments deserved some success. Unfortunately, they met with too much success: these dichotomous issues have been decided, again conclusively, with answers diametrically opposed to the former set. Linguists are still faced with preconceived orthodox answers to very basic questions. Too often, these questions, answers decided by prior assumption, are never even asked. Still rarer is a reconsideration of their consequences. Worse, if they are asked by a non-establishmentarian, they are ignored or dismissed ex cathedra. Lamb 1964 questioned the appropriateness of rewrite or mutation rules to synchronic description as well as the logical necessity of (extrinsic) order of application of rules and was righteously refuted by Postal 1968. No such polemic has been mounted against Koutsoudas (e.g. 1972), though King (ms) engaged in debate by invitation. Nor has this tendency been eliminated. Maher's humorously critical review (1973) of Stockwell & Macauley 1972 met with immediate response from Zwicky (1973).

Maher (1973:52) uses the term 're-inventing the wheel' in regard to several specific examples of transformational-generative theorizing. He is quite correct, as the juxtaposition of Lamb 1964 with Koutsoudas, Sanders & Noll 1974 shows. For another example consider Hooper 1973. The conclusions reached in this article are generally well taken: it is a 'pioneering' effort directed at demonstrating the necessity of the description of the syllable in phonology. This ground-breaking was consummated in apparent ignorance of Haugen 1956, Lamb 1966, or Sampson 1970, not to mention a substantial series of older studies.

Thus it seems insufficient for me merely to refute one set of arguments and supply an alternative. The historical development of the polemic must be included. Lamb 1964, which was written to counter transformational-generative theorizing on several issues was an undeserved failure. But rightly or wrongly, those issues are still considered closed, and dogmatic answers to basic questions are considered sufficient.

Therefore I wish at this point to consider two descriptive problems: contrast in phonology and the compatibility of logic with the semiotic function of deixis in syntax and linguistic semantics. The phonology problem concerns voice assimilation within obstruent clusters in Russian, and the syntax problem concerns clause negation in simple transitive sentences

in Russian. The phonology question has been argued almost continually since 1959, generally using only Halle's original data. The general establishmentarian consensus seems to be that Halle 1959 provided both the best description for this particular problem and the final solution to the theoretical issue.

Conversely, the syntax-semantics problem is representative of the split within the transformational-generative ranks. The deep- or remote-structure nature of negation has been studied intensively in the last few years, especially in G. Lakoff 1970b. The mode of his solution, though not universally accepted in Massachusetts, involves a dizzying combination of negation as a 'higher verb' with Neg-raising and Neg-lowering transforms. Jackendoff 1971 undercuts the syntactic arguments Lakoff adduced. But the lexicalist framework simply cannot account or the logical effects of negation.

Both these problems are ripe for reexamination from a different point of view. In both cases the recent history of the polemic is given and solutions to the matters at issue are presented from scratch. Finally, the solutions are considered in the light of the larger polemic. In turn, the light they shed on questions of more general import is shown. The directionality of language, the means of specifying process information in linguistic description, and the distinc ions between base form with transformation or explicitly stratified structure with realizational relations are all considered.

To effect this rather ambitious aim, the specific descriptive questions must be answered in ways which satisfy the structural exigencies without transmogrifying them into some model-specific shape. In the course of this study, stratificational theory is used as the descriptive vehicle because of its position on these greater issues. Of all the dichotomies considered, no choice has been made by virtue of prior assumption alone (cf. Lamb in press, Sampson 1970, and Sullivan 1972). Thus the model does not arbitrarily and misleadingly modify the solution.

1. The archiphoneme, contrast, and biuniqueness in phonology

I. Introduction

The Neo-Bloomfieldian requirement of biuniqueness in phonological description was first attacked by Halle (1959:21-24). The term itself stems from Hockett (1951:340). Chomsky (1964, 1966) continued the attack. At about the same time Lamb (1973:131-39; originally appeared in 1966) sprang to the defense of biuniqueness, though not, as Postal (1968:50-52 et passim) seems to suggest, to the defense of structuralist phonology in general. The question, using Halle's original example, has been argued several times since then, reappearing in Postal 1968, Johns 1969,

Sullivan 1969, Sampson 1970, and Lockwood 1972. Not all the arguments are readily available in print, and it will be useful to put them together and see what general conclusions can be drawn.

More specifically, as Lamb 1973(1966) pointed out, the topic at issue turned out to be the existence of the classical or Neo-Bloomfieldian phoneme. Hence, it was concerned with the existence of a level of contrast in phonology. The questions raised by Halle, Chomsky, and Postal can be answered, and a phonological theory which provides descriptions with a biunique level of contrast and which predicts all non-contrastive features can be constructed. It appears, however, that the classical phonemic level is too close to sound to fulfill all these conditions. A satisfactory phonological model requires a somewhat more abstract level of contrast and includes the Prague school archiphoneme. Stratificational theory is shown to provide both of these explicitly. Moreover, the archiphoneme in stratificational description is shown to satisfy both the neutralizing and feature definitions of the archiphoneme given in Trubetzkoy 1969.

II. The death struggle of the classical phoneme

The classical phoneme should be understood as a particular manifestation of the phonemic level or level of contrast in Neo-Bloomfieldian phonology. One of the required characteristics of a classical phonemic description is biuniqueness, as defined by Hockett (1951:340).

Discounting free variation, the biuniqueness condition requires that a stretch of sound be uniquely represented by a sequence of phonemes and the converse, that a sequence of phonemes represents some unique stretch of sound. This condition is a corollary of the distinctiveness principle. But distinctiveness is a necessity for communication. That is to say, a communication system must have a means of distinguishing unambiguously between the contrastive (phonemic, meaning-bearing) and the non-contrastive (phonetic, incidental 'noise'). Such an ability is central to the communication function: it is the means by which a system seeks out a meaning or set of meanings from a burst of sound.

Halle's criticism (1959:22-23, cf. also Lamb 1973:135-36) runs as follows:

> In Russian, voicing is distinctive for all obstruents except /c/, /č/, and /x/, which do not possess voiced cognates. These three obstruents are voiceless unless followed by a voiced obstruent, in which case they are voiced. At the end of the word, however, this is true of all Russian obstruents: they are voiceless, unless the following word begins with a voiced obstruent, in which case they are voiced. E.g. [m'ok l,i] 'was (he) getting wet', but [m'og bɨ] 'were (he) getting wet'; [ž'eč, l,i] 'should one burn?', but [ž'eǰ, bɨ] 'were one to burn'.

> In a phonological representation which satisfies [the biuniqueness condition] the quoted utterances would be symbolized as follows: /m'ok l,i/, /m'og bi/, /ž'eč l,i/, /ž'eč bi/. Moreover, a rule would be required stating that obstruents lacking voiced cognates--i.e. /c/, /č/, and /x/--are voiced in position before voiced obstruents. Since this, however, is true of all obstruents, the net effect of [the biuniqueness condition] would be a splitting up of the obstruents into to classes and the addition of a special rules. If [the biuniqueness condition] is dropped, the four utterances would be symbolized as follows: {m'ok l,i}, {m'ok bi}, {ž'eč l,i}, {ž'eč bi}, and the above rule could be generalized to cover all obstruents, instead of only {c}, {č}, and {x}. It is evident that [the biuniqueness condition] involves a significant increase in the complexity of the representation.

But as Lamb (1973:138-39) points out, Halle's assessment of the particular description he was attacking as inefficient is correct. However, the description referred to is not itself biunique. Specifically, 'phonemic' voice is not treated as a phoneme. Lamb (1973:136-38) therefore suggests describing the obstruents in terms of a combination of obstruent with (optional) devoicing phoneme *h*. Then Halle's data could be written morphophonemically *mogh l,i*, *mogh bi*, *zežh l,i*, *zežh bi*, and phonemically as /mogh l,i/, /mog bi/, /zežh l,i/, and /zež bi/. In this description, which is biunique, the obstruents are all in the same class, and no special rule is needed for /c/, /č/, and /x/, thus answering Halle's attack on biuniqueness.

In fact, as Lockwood (1972:193) points out, the offending principle is the principle of non-suspendibility of contrast or 'once a phoneme, always a phoneme'. This principle effectively asserts that a contrast in one environment implies ontrast in all environments. Such a claim is simply untenable. Moreover, though not all neo-Bloomfieldian linguists held the principle of non-suspendibility dear, classical phonemic theory never developed a unified principle of suspendibility of contrast.

Now Chomsky (1966:80-82) rejoined that Lamb's position agrees with Halle's. This is true if it is restricted to the rejection, as inefficient, of the specific phonemicization of Russian under consideration. Conversely, Lamb most emphatically did not reject biuniqueness at all, either in theory or in practice. (I can personally testify to the stringency with which he required its application to the solutions of problems assigned in his phonology classes.)

However, Lamb did not explicitly develop a theory of suspendibility of contrast in phonology. Suspension of contrast in phonology is, to Lamb, simply a specific instance of the general, language-wide phenomenon of neutralization, expressed by an upward 'or' relation. The unwary therefore concluded that Lamb was implicitly claiming that the *ž* of *žež bi* is phonemic in the classical sense. This is a significant point which is considered in greater detail below.

Postal (1968:35-50) criticized Lamb's solution much more perceptively. He states that the choice of marked voiceless obstruents ultimately causes Lamb's solution to violate what Postal (1968:43) calls condition 3.14: 'Maximal reduction or diversification ... every phonetic feature which can be predicted in terms of purely phonetic context is eliminated and not marked in the phonemic representation.'

The source of Postal's objection to Lamb's description lies in Lamb's choice of the unvoiced obstruents as the marked members of the voiced-voiceless opposition. To a committed user of the MIT brand of universal distinctive feature markings, this is doubly objectionable. Distinctive feature markings assign the positive mark to the voiced member of the opposition, for Russian as well as for all other languages. The arguments concerning this conclusion go back to Jakobson, Fant & Halle 1952 and need not be rehashed here. However, Jakobson himself has discussed languages for which the voiced obstruents are the unmarked, though in these cases he prefers the correlation tense-lax (cf. Jakobson, Fant & Halle 1952:36-39). For example, by all relevant criteria (neutralization, assimilation, statistical occurrence, morphophonemic alternation, etc.), the unmarked members of the voiced-voiceless opposition in Welsh are the voiced.[1] In any case, this is a minor objection. In spite of his misgivings on this score, Postal is willing to concede the point. He later contends (Postal 1968:50) that marking the voiced will not help the situation, but he seems less sure of his grounds here.

Postal's main objection is much more serious, and refers to 'condition 3.14'. He points out that Lamb's description requires the phonemic representation of *c*, *č*, and *x* as /ʒh/, /ǰh/, and /γh/, respectively. Now /ʒ/, /j/, and /γ/ do not occur in Russian. Thus, the fact that /c/, /č/, and /x/ are unvoiced should be predicted by the phonology. Since it is not, condition 3.14 is violated. The situation is also anomalous from the point of view of markedness theory. Lamb has said (personal communication) that this is a problem, but that he described the data both ways, i.e. with marked voiced and marked unvoiced obstruents, and determined that the description he presented is minimally simpler. This weakness in Lamb's description stems from the fact that he does not personally command the data of Russian phonology, and he simply accepted the data presented by Halle as complete.[2]

In any case, whether nobody read Lamb's rebuttal of Halle or whether Chomsky's rejoinder (1966) or Postal's objections (1968) were considered conclusive, the biunique level and the classical phoneme were missing from transformational-generative phonology. The classical phoneme never really passed from the scene (cf. V. B. Makkai 1972:293-95), but it had no influence on generative descriptions. Johns 1972 (first appeared in 1969) argued in vain for the resurrection of a phonemic level, including a principle of suspendibility (Johns 1972:549). Sampson (1970: 593) explained why Johns (and Lamb) failed in their objective: the presence or absence of a phonemic level 'does not affect the claim made by the grammar as to what is the set of sentences in the language.'

Thus, as far as transformational phonology was concerned, the classical phoneme: <u>requiescat in pace</u>.

III. The archiphoneme and a transformational-generative description

In fact, very little has been written about the archiphoneme from the viewpoint of transformational-generative phonology. The main thrust of this section, therefore, is doubled. First, a more complete description of the voice assimilation problem is provided. This permits a more accurate comparison of the generative description with the stratificational than is possible with Halle's description. Arguments can be adduced with reference to methods, complexity, 'naturalness', and consequences (or 'claims') of the two descriptions.

Second, serious consideration is given to the possibility that an archiphonemic level is discoverable in some generative model currently in use. I conclude that the archiphoneme does not occur in either the 1959 or SPE model. Griffen 1974 considers what changes would be necessary in the model(s) to accommodate the archiphoneme and discusses the consequences of such changes in an illuminating fashion.

But Halle has also considered this question and has concluded that generative phonology does accommodate the archiphoneme, at least implicitly. It is therefore necessary at this point to assess Halle's comments and determine whether a biunique description could be developed in either the 1959 or the SPE framework.

Consider first Trubetzkoy's definition of the archiphoneme as 'the sum of distinctive properties that two phonemes have in common' (Trubetzkoy 1969:79). He clarifies this statement with the comment that the unmarked member of the distinctive opposition is the one which 'functions as the archiphoneme in the position of neutralization' (Trubetzkoy 1969: 147). Both statements must be kept in mind during the following discussion. It is shown in section IV below that both apply to privative, bilateral oppositions which can be characterized by the presence vs. the absence of a particular mark. For convenience, the first definition is referred to as the feature definition, the second as the neutralizing definition.

Now Halle (1959:39) states that the morphoneme when not fully specified is 'analogous to' an archiphoneme. But this statement is in error or is at best imprecise. It is almost true of an obstruent in non-final position in an obstruent cluster. In this position, obstruent morphonemes are left unspecified for voice preliminarily, i.e. they receive a '0 voice' marking. Voice is then supplied via an alpha-voicing rule. In the more recent approach (Chomsky & Halle 1968: cf. Ch. 9), the preliminary specification would be for 'U voice', but the procedure is basically unchanged. A marking of '+ voice' or '- voice' must still be supplied.

In this position of neutralization, the morphonemes *t* and *d* would both be specified as in figure 1 below (Halle 1959:45). Ignoring diffusivity and continuance, the morphoneme must be supplied with '+ voice' or '- voice', as stated above. The feature definition of the archiphoneme would be satisfied if the voice feature, whether +, -, or 0, were simply left out of consideration. But the thing thus specified is nei-

- vocalic
+ consonantal
0 diffuse
- compact
- low tonality
- strident
- nasal
0 continuant
0 voiced

Figure 1

ther *t* nor *d*, and the neutralizing definition would seem to require one or the other morphoneme in this particular case. The latter definition is therefore unfulfilled. Nor is the feature definition explicitly fulfilled, since there is no provision in the transformational framework for actively ignoring any of the features, a liability of their intended universality. Moreover, it is difficult to see how the neutralizing definition could possibly be fulfilled in the framework of Chomsky & Halle 1968 or Postal 1968 or indeed any framework which lacks an independent phonological base or phonotactics (cf. Sampson 1970). It is likely that a theory which does incorporate the archiphoneme directly would exhibit extensive differences in structure from the transformational models mentioned, just as Griffen 1974 concludes.

Let us consider now a more complete treatment of voice assimilation. The mechanism necessary requires a minimum of three rules. These are presented in the Halle 1959 framework, with some Lightnerian modifications. Because of the fact that obstruent clusters are all voiced or unvoiced, the rules must be applied iteratively.

(1) + vce → - vce/ $\_C_0\#$ (word-final devoicing)

(2) α vce → $-\alpha$ vce/ $\_-\alpha$ vce (morpheme-final)

(3) 0 vce → α vce/ $\_\alpha$ vce (intra-morphemic cluster)

It should be understood that these rules refer only to obstruents. This raises another problem, since there is no straight-forward way of classifying obstruents in the Halle 1959 framework. They would be distinguished from e.g. nasals by non-nasality and from sonorants by the fact that voice can be contrastive. But that is the problem we are trying to describe, so the issue is ignored. The SPE framework does allow direct reference to obstruents. Moreover, (1) is insufficient as written because it ignores the fact that there is no word-final devoicing in clusters ending in a sonorant. This does not affect the main argument and can be ignored for the sake of simplicity.

Now it would seem that (3) is secure as a rule independent from the

other two. It supplies a voice marking to an obstruent in an unmarked position, where none had been given. But the word- and morpheme-final rules rewrite actual plusses or minuses. Two comments are required. (1) is not sufficient, because it does not, as written, account for voicing in Halle's [ž'eǰ bɨ] example. (2) is also unsatisfactory because it is not fully independent from (1). A word boundary always coincides with a morpheme boundary.

These objections can be satisfied most simply by reformulating rule (2) to include (optionally) the word boundary. This is given in (4). However, we cannot do away with (1). It is necessary to ensure

(4) α vce $\rightarrow$ -α vce/ _(#)-α vce

that word-final obstruents are unvoiced if no voiced obstruent begins the following word. Clearly, this description is not biunique. Moreover, it only appears to have just three rules. Logically speaking, an alpha-voicing rule is actually two rules which have been collapsed by a notational device. Worse, they must be extrinsically ordered (3)-(1)-(4) to predict the correct voice character of an expression like *príezd v zdánie* 'arrival at the building'. The details of the derivation of the voice specification of the *zd v zd* cluster are given in table 1. Note that the symbol # represents the word boundary and the = represents the prefix-preposition boundary. Note also that the issue of *v* has been ignored (cf. Andersen 1969). This would complicate the problem still more.

underlying morphonemes	z	d	#	v	=	z	d
voice marking (MS rules)	0	+	#	0	=	0	+
(3)	+					+	
(3) (iteration)				+			
(1)		-					
(1) (iteration)	-						
(4)		+					
(4) (iteration)	+						
surface forms	+	+	#	+	=	+	+

Table 1

The extrinsic ordering is necessitated by the environments of rules (1) and (4). Both are obligatory whenever the structural conditions are

met. Every time the conditions for (4) are met, the conditions for (1) are met. But the converse is not true. There are times when (1) applies and (4) cannot. For example, in the expression *príezd na póčtu* 'arrival at the post box', what is written *zd* is pronounced [st]. This shows the need to account for devoicing just in case re-voicing is not applicable. But this is no problem. However, if the rules were applied in the opposite order, (1) would everywhere obviate the effect of (4). (4) would not be blocked; its output would be rewritten. Thus it does not come under any of Koutsoudas' injunctions, and extrinsic order must be specified.

Now the problem might be soluble in several other ways. Different rules could be drawn up and different approaches used. If desired, word boundary could be treated as some kind of non-segmental segment, as some have suggested. But none of the solutions within this framework would be biunique. Nor is there a unified mechanism (or 'metric') for choosing among them, whether on the basis of simplicity, naturalness, or what.

But in any case, the solution necessitates more than Halle 1959 mentioned, and more than I have covered here (cf. the comments accompanying table 1 above). The description provided herein does take inputs and predict the outputs in cases that Halle's description does not consider. The surface forms are correct, even if the derivation is sometimes tortuous: the voice marking of the first *d* is changed twice and of the first *z* three times. I find such a description hopelessly ad hoc. The fault for that may be mine. However, any phonological description which is not biunique destroys or ignores information about the code. This in itself is grounds for a reexamination of the methods of description in hopes of improving the possibilities for communication.

In this context we turn to a description which retains contrast and is biunique.

IV. The archiphoneme and phonemic voice

Sullivan (1969:260-62) reworked the problem of phonemic voice from scratch. This was necessitated by the fact that the data given in Halle 1959 are misleadingly incomplete and had led Lamb to an incorrect conclusion, as mentioned above. It became apparent only later that the description, formulated in purely stratificational terms, had made consistent and explicit use of the archiphoneme.

Recall that Trubetzkoy (1969:79 and 147) gave two relevant definitions of the archiphoneme. To repeat: the feature definition states that the archiphoneme is 'the sum of distinctive properties that two phonemes have in common'. The neutralizing definition states that the unmarked member of the distinctive opposition is the one which 'functions as the archiphoneme in the position of neutralization.' This is

a sophisticated and often misunderstood concept. But these two definitions should serve to delimit the concept once and for all. However, they have not. David C. Bennett (personal communication) has pointed out that this is probably because there is nothing in the neutralizing definition which would block the recognition of an archiphoneme for every pair of phonemes sharing at least one distinctive feature. But by assuming that both definitions be fulfilled before a given entity can be designated an archiphoneme, the dilemma can be resolved.

In sum, then, the feature and neutralizing definitions should be understood as referring to manifestations of the same phenomenon on different levels of structure. This is quite a sophisticated concept and has led, as remarked, to a good deal of misunderstanding about its precise meaning and application.

The phonology of obstruents and obstruent clusters in Russian is given in figure 2 on the following page. This description is somewhat simplified with regard to the phonology of other phenomena, e.g. substitutive softening, but not with regard to voice. Consider the diagram. The diamond nodes represent the intersection of the realizational and tactic portions of the phonology. The realizational portion is related upward (northwest from the diamonds) toward meaning and downward (southeast from the diamonds) toward sound. The diamonds are also related to positions higher (northeast) and lower (southwest) in the phonotactics or syntax of phonemes. At the upper left is the inventory of morphons (roughly equivalent to morphophonemes). At the bottom is the inventory of phonons (singulary, articulatory phonetic features) by which the morphons are realized. The intermediate level of diamonds represents the phonemic level. That is to say, the diamonds are the phonemes. They are defined at the level of contrast. The realizational portion therefore gives the inventory of phonemes, which are, from another point of view, the basic elements on the phonemic stratum, and relates them to the adjacent strata. The relation of morphons to phonemes involves a certain amount of potential alternation and neutralization (downward and upward 'or' nodes respectively), with a reduction from fifteen morphons to ten phonemes. The phonemes are realized below the tactics by only eight features.

The phonotactics specifies arrangements, environments of occurrence, and linear sequences of phonemes, and controls the output, whether of phonon matrices during encoding or of morphons during decoding. The lowest phonotactic level shows the set of stops, characterized realizationally by their relation to the phonon *Cl*, and the set of spirants, characterized by their relation to the phonon *Sp*. These sets are also characterized by their tactic distribution. The set of possible obstruent clusters is defined by the lower 'and' node. It states that an obstruent cluster may have a maximum of three positions. In the first position may be either a stop or a spirant, but the second position must have a spirant and the third must have a stop. Thus we get *kst* in *kstáti* 'by the way' and *fst* in *fstréča* 'meeting'. The clusters are formed via purely phonological criteria and without regard to morpho-

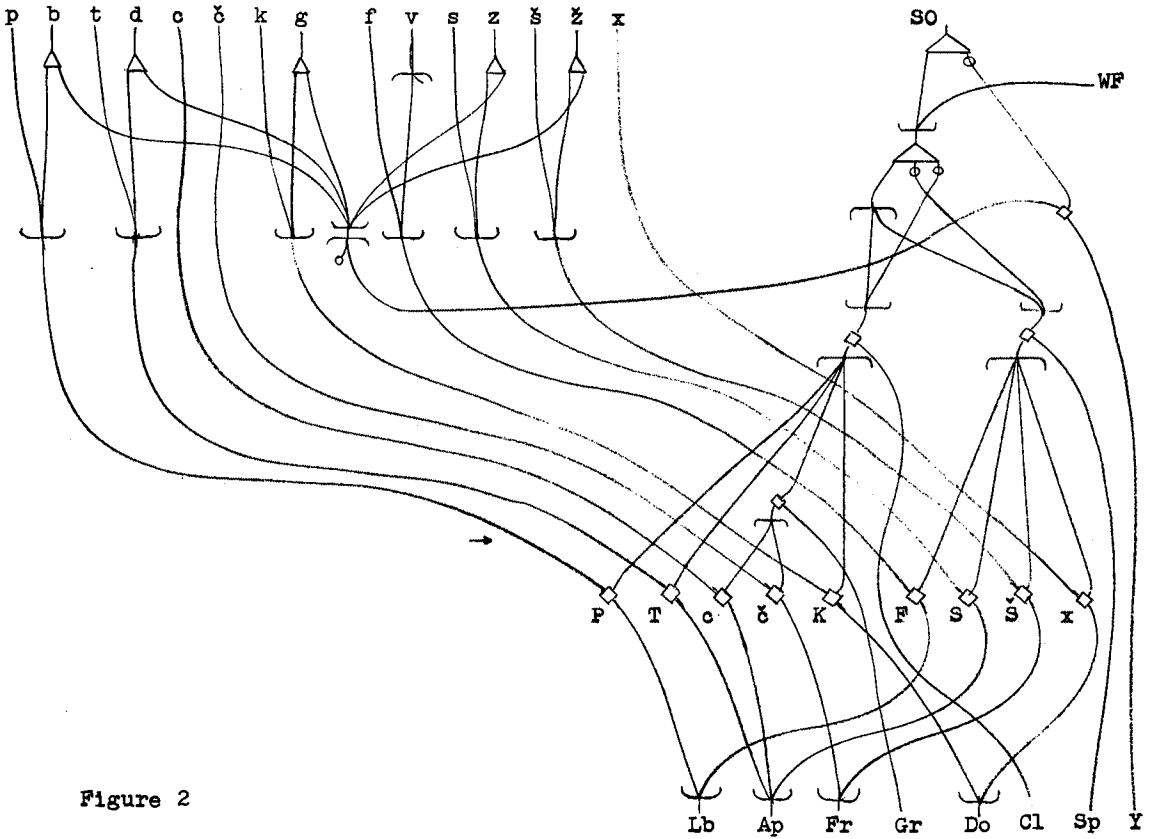

Figure 2

logical information, though any claim for intervening morpheme boundaries in the examples chosen would be at best moot. The circles around the second and third lines indicate optional occurrence. Thus if we take the first two lines but not the third line, the description predicts stop-spirant and spirant-spirant clusters: cf. *pš* in *pšeníca* 'grain' and *sf* in *sféra* 'sphere'. If we take the first and third lines but not the second line, the description predicts stop-stop and spirant-stop clusters: cf. *pt* in *ptíca* 'bird' and *ft* in *ftorój* 'second'. Taking only the first line and not the others predicts that any obstruent may occur alone.

Above this node there is an upward 'or' which states that these obstruents or clusters may occur in two positions: word-final (WF) or syllable-onset (SO). The topmost 'and' indicates that obstruents or clusters in SO position may occur with or without contrastive voice (Y): cf. *fst* in *fstréča* 'meeting' and *vzd* in *vzdór* 'nonsense'. The same holds for non-initial syllables: cf. *st* in *mésto* 'place' and *zd* in *gnezdó* 'nest'. But the fact that WF is related below the level at which voice is specified predicts the lack of a voice contrast in word-final position: cf. the genitive plurals *mést* and *gnézd*, both with [st]. Note that what the neo-Bloomfieldians called 'phonemic' voice is indeed a phoneme here. That is, it is an element (a diamond) on the phonemic stratum. However, unlike the obstruent phonemes, it cannot occur alone. It must have been preceded by the occurrence of an obstruent phoneme. It is therefore a dependent phoneme, i.e. one which occurs only in a dependency relation with other phonemes in the phonotactics.

To illustrate the mechanics of the encoding process with relation to the obstruent cluster, consider the data in table 2.

		t:d	
		podobrát'	'choose(perfective)'
		otobrát'	'select(perfective)'
		podojtí	'approach(perfective)'
		otojtí	'move away(perfective)'
/db/ only		/tx/ only	
podbirát'	'choose(imperfective)'	podxodít'	'approach(imperfective)'
otbirát'	'select(imperfective)'	otxodít'	'move away(imperfective)'

Table 2

The above data represent more information that Halle did not discuss. He discussed only the simpler word-boundary situation, wherein the obstruents are unvoiced unless a following word begins with a voiced obstruent or obstruent cluster. Voice is never contrastive. But the situation considered herein is more complex in that voice may be contrastive. Again, the problem of /v/ is ignored.

	podobrat'	otobrat'	podbirat'	otbirat'	podxodit'	otxodit'
morphons	d	t	db	tb	dx	tx
MAP	TY	T	TYPY	TPY	TYx	Tx
PT	TY	T	TØPY	TPY	TØx	Tx
PSP	Cl_Y Ap	Cl Ap	$ClCl_Y$ ApLb		ClSp ApDo	
HPT	Cl Ap Y	Cl Ap	ClCl ApLb Y		ClSp ApDo	

N. B. Left-to-right printing indicates serial occurrence; vertical printing indicates simultaneous occurrence.

Table 3

The steps in the process are summarized in table 3. The rows in table 3 correspond to realizational levels of figure 2, reading from the top down. In the second row, the morphonic realizations of the segments and clusters underconsideration are given. The prefixes *ot* 'away from' and *pod* 'toward' may differ in the morpheme-final segment (cf. table 2). However, the contrast applies only if a vowel or sonant follows. If an obstruent follows, there is absolute neutralization. Both are realized as /t/ if an unvoiced obstruent follows or as /d/ if a voiced obstruent follows. In the morphonic alternation pattern (MAP), the morphon *d* is realized as the sequence *TY*, where *T* is the archiphoneme of neutralization between *t* and *d*, and *Y* symbolizes phonemic (contrastive) voice, the distinguishing mark. The assumption implicit here is that a *d* is nothing more or less than a voiced *t*, i.e. that it is the logical sum of *T* and *Y*. Similarly *z* is a voiced *s*, *b* a voiced *p*, etc.

However, it should be understood that these are only labels to lines (relationships) in figure 2 and have no independent reality. In this case, *d* is not necessarily realized as *TY*. The morphon *d* is necessarily realized

by *T* but only potentially by *Y*. In other words, *d* is realized as *T* and, if possible, *Y*. The labels are provided for convenience. It is a crucial distinction between morphonic and phonemic voice that the former is potential only; the latter is actual.

Now the encoding can be effected biuniquely. The phonotactics allow phonemic voice to occur only at the end of an obstruent or obstruent cluster in syllable on et position. Elsewhere morphonic voice is realized as phonemic zero, i.e. it is 'squeezed out' of the phonology and never generated by the phonotactics. Since the features are singulary, the morphon *t*, the archiphoneme *T*, and the morphon *d* (with voiced realized as zero) are phonemically identical and hence represented by the same set of features. This is the central question in the test for biuniqueness.

The phonotactic level is the level at which contrast is specified. This implies two central functions for the phonotactics with regard to contrast. First, the sum set of contrastive features is fully specified. Second, the environment in which each feature occurs is stated. From this level on down to the actual production of sound, there is no distinction between the realizations of *tb* and *db*, *tx* and *dx*, as the biuniqueness criterion demands. Singulary features require no alpha-switching rules or iterative application of rules. If *Y* is not there, the cluster is unvoiced.

If it is there phonemically, the hypophonotactics realign it as a cluster prosody, i.e. it dominates the entire series of obstruents. The last line of table 3 states what happens in the hypophonotactics for this particular example. The general graphic description is given in figure 3. To handle the data Halle described, it is necessary only to define this prosody as (potentially) spanning clusters on either side of a word boundary.

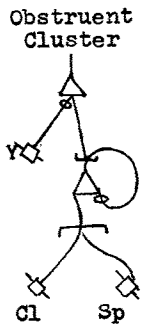

Figure 3

With respect to the archiphoneme: consider first the feature definition. The data in table 2 exhibit the pattern, and table 3 and figures 2 and 3 show how it is effected in a stratificational description. The problem of voice, whether for a single segment or for a cluster, is handled at the highest tactic level described. That level tells whether the segment is a *p* or *b*, *s* or *z*, etc. Travelling down the left-hand branch of the tactic diagram from that point, it matters not whether *Y* is present, i.e. whether the morphon *p* or the morphon *b* was active. In either case, the features *Cl·Lb* are provided. Thus the feature definition is fulfilled with no need to worry about the ultimate significance of a '0 voice' marking.

Consider now the neutralizing definition of the archiphoneme. The 'position of neutralization' is described in the phonotactics. It is the diamond (and whatever it is related to) at the bottom of the phonotactics which is found in these positions of neutralization (cf. the tactic portion of figure 2). But the diamond is activated during the encoding process via the morphonic alternation pattern. Following the lines upward from the diamonds to the morphonic alternation pattern (northwest), we find first upward 'or' nodes, which explicitly state that there is a neutralization of *t* and *d* as one element, designated *T* in table 3; of *s* and *z* as *S*, etc. Thus these lines relating the diamonds at the bottom of the phonotactics to the upward 'or' nodes, which represent the neutralization of 'voiced' and 'unvoiced' morphons in the morphonic alternation pattern (cf. the arrow to the lower left in figure 2), fulfill the neutralizing definition of the archiphoneme. In addition, these lines effect, during the encoding process, the activation of the diamonds at the bottom of the phonotactics. In turn, these diamonds determine the path which will be chosen in the tactics. This tactic path is the relationship which specifies which phonons the archiphoneme is related to in the phonemic sign pattern.

Thus, in stratificational terms, the archiphoneme can be defined as a line connecting a diamond at the bottom of the phonotactics to an upward 'or' in the morphonic alternation pattern and the set of phonons whose activation is effected by the activation of this diamond during the encoding process. Simultaneously, since a stratificational diagram is not restricted to encoding alone, the converse definition is equally valid: an archiphoneme is a set of phonons which uniquely activate a line leading upward from a diamond at the bottom of the phonotactics to an upward 'or' in the morphonic alternation pattern during the decoding process.

Remember that no claim for the universal applicability of the archiphoneme to all languages is made. Its applicability to the phonological description of a given language is open to empirical investigation. But descriptions of languages of this sort, e.g. Vietnamese (cf. Sampson 1969 and 1970) and Aymara, do not exhibit patterns of relationships of the sort described in the above definition.

But in any case, it should be clear that the description of Russian phonology can be effected with the use of the archiphoneme, and that stratificational phonology accounts for this patterning directly.

V. Contrasting the descriptions

In spite of the radical differences in underlying assumptions and mode of description, the two descriptions provided above can be profitably compared in two ways: in what the descriptions can do and in the complexity of processing things both descriptions account for.

Both descriptions 1) provide minimally marked underlying forms; 2) correctly predict a maximal number of surface features; and 3) provide an explanation of the means of getting from underlying to surface forms. In addition, the stratificational description 4) maintains a level of biunique contrast and 5) provides an explanation of the means of decoding from surface to underlying forms. Moreover, it should be remarked that the two descriptions encode by radically differing modes.

But the picture is still not complete. It is immediately apparent upon inspection that though a stratificational description of the phonology of a particular language makes no a priori claims to universality, it is much more highly elaborated. Of course, if claims (4) and (5) above are true, the additional elaboration is irrelevant, because the stratificational description accounts for roughly twice the information, i.e. for decoding as well as encoding.

However, the ways in which the two account for encoding is also of interest. The transformational description predicts surface features by a set of rewrite rules, at least some of which must be applied iteratively. Some rules may even wipe out the effects of earlier rules. Mutation rules as a descriptive mode wed the description of a linguistic process, here encoding, to the (implicit) structural description of the forms. Because the encoding process is chosen, no biunique level is necessary for consistent operation. Because there is no biunique level, decoding cannot be effected. Because the description encodes but cannot decode, the linguistic structure described is uni-directional.

The stratificational description is effected by means of a set of relationships which explicitly describe the structure. Separate from the structure is the information telling how the network of relations is to be processed. In the problem under consideration, the transformational-generative description requires three logically undifferentiated rules (four if the alpha-voicing convention is given its actual value). That they are logically undifferentiated is shown by the fact that the significant elements of the input alphabet are identical in inventory to the significant elements of the output alphabet. Only the distribution is changed. Thus, though there is some prediction included, the logical level of 'base' and 'surface' forms is the same. Now these rules rewrite an underlying something as '+ voice' or '- voice' in some set of morphological and phonological environments. They are applied whenever the structural descriptions are fulfilled.

By contrast, the stratificational description is split into two logically different parts, each with its own function. First is the pho-

nemic level, on which suspension of contrast is described. Written Y/ø, suspension of contrast is referred to above as the zero realization of the morphon *Y*, which represents (potential) voice. But this statement might be misleading. In fact the suspension of contrast is automatic whenever the morphon does not fit into the phonotactics and cannot be realized as phonemic voice. The morphon *Y* is only potentially contrastive. Unless it can be actualized as the phoneme *Y*, it is omitted, i.e. it is never generated. For this reason it need never be 'rewritten'.

Secondly, there is voice assimilation, i.e. the extension of the domain of the single phonemic occurrence of voice to the entire cluster (including, if necessary, the word boundary and any word-final obstruents in the preceding word).

Thus we have one suspension of contrast and one hierarchic shift: logically speaking, two rules. Note that these two realizations are not in the form of Lightner's iterative rules. There are several differences. First, Lightner does not distinguish between loss of contrast and assimilation, an incidental effect of his treatment of binary marking. Second, the domain of Lightner's rules seems to be delimited by word boundaries, insofar as I have seen them, and some assimilative realizations are not so limited. But this is not a particularly crucial problem. Far more important is the fact that Lightner generates '0 voice' (or its equivalent) even in those areas where voice is never contrastive, then changes it to '+ voice' or '-voice'. Finally, he ends up with a string of plusses or minuses. The superfluous generation of '0 voice' remarked on above is another effect of his treatment of 'binary' marking; he should generate *nothing*. Moreover, as remarked above, I treat voice as a cluster prosody (one occurrence of voice dominating a cluster hierarchically), not as a string of distinctive feature markings.

Note also that, as Trubetzkoy (1969:80-81) suggests, voice in such a construction is treated as the presence vs. the absence of the mark ; either the is present, and the segment or cluster is voiced, or it is lacking, and the segment or cluster is unvoiced. There is no need for a special mark '- voice' in the realizational structure, i.e. there is no phonon or feature '- voice'. Nor is there a need for a marking '0 voice' to show potential, one way or the other. That potential is implicit in the tactic structures, and is automatically realized, and realized correctly, during the encoding process. A separate specification of '0 voice' would therefore be wasteful. Moreover, the stratificational realizational relationships are formulated on purely phonological grounds, and the transformational description requires reference to morphological information in the statement of underlying forms. There is no objection on an a priori basis to the inclusion of morphological information in phonology. But it seems clear that a phonological description without such information is simpler than and preferable to one with it.

Now it might be objected that the cluster definition into which the phonemic occurrence of voice is fitted in figure 2 is a tremendously complex element of the stratificational description. In fact it is not.

This general description of cluster structure is merely part of the description of the syllable. The syllable itself is a necessary unit of actual language use (cf. A. Makkai 1973). But Halle (1959:57) includes a set of morpheme structure constraints. These are what determine the zeroes in the underlying forms. The description of the Russian syllable (Sullivan 1969:fig. 68) is more fully generalized than Halle's MS constraints, simpler, and actually predicts the occurrence of clusters that Halle's rules do not. In addition, contrast is specified and the occurrence of features which are not contrastive is predicted. The syllable and its functions are complex, to be sure. But they are less complex and can do more than MS constraints.

Finally, because there is a biunique level of contrast, the stratificational description can decode as well as encode.[3]

Thus it should be clear that the stratificational description in general defines a greater set of functions than the transformational and provides, for the problem considered, a simpler solution.

2. Deictic function and the logic of sentence negation in Russian

I. Introduction

The simplest and most common subject-verb-object construction in Russian consists of a nominative subject, a verb with the appropriate desinence, and an accus tive object representing an agent-act-patient event. This is the construction of sentence (5). This simple sentence

(5) Iván čitáet knígu. 'John read book(acc)'

has several possible equivalents in English, e.g. 'John is reading a book', 'John is reading the book', 'John is reading a book (i.e. not a magazine)', etc. All these meanings are possible for (5), in isolation, at least. If it is given with a context as a part of a greater discourse block, most of the possibilities may be sufficiently unlikely as to be ignored by the listener, with one of the remaining possibilities being assumed as the most likely.[4]

But if the verb in (5) is negated, there are two possibilities: the object may be in the genitive, as in (6), or it may remain in the accusative, as in (7). Both (6) and (7) are negated equivalents of (5), but

(6) Iván ne čitáet knígi. 'John not read book(gen)'

(7) Ivǎn ne čitáet knígu. 'John not read book(acc)'

they are not equivalent to each other. One possibility is that (6) negates one of the senses of (5) and (7) another, but the situation is not that simple. There are several senses of (5), not just two. Some other descriptive approach must be used. Attempts have been made to ascribe the difference to logical stress (Morison 1959 and 1964) or to a highly complex semantic-syntactic subcategorization system (Davison 1967). Morison's description does not work, and Davison's is too complex to be psychologically plausible (cf. below).

Sullivan 1972 reanalyzed the problem, producing an integrated, stratified description based on the intersection of the semantic specification of simple deixis with the syntactic specification of morphological case. Sullivan (in press c) analyzed the situation yet again from the viewpoint of the logic of negation. This description is also stratified and predicts the difference between (6) and (7) on the basis of the semantic distinction between categorical negation (i.e. of the entire predicate relation), which dominates the act-patient relationship and evokes the genitive of negation, and act-specific negation, which does not. Act-specific negation does not dominate the patient logically. Therefore, the negatively-favored genitive is not evoked and accusative is preserved.

The two descriptions seem at first to be in conflict. The deictic description claims a marked accusative in the environment of negation; the logic-of-negation description 'favors' the genitive. Worse, native speakers find both descriptions convincing. But the anomalous markings can be reconciled. The two descriptions are shown to be complementary. They are integrable into a single, stratified description.

These findings on the semiotic and logical functions of negation are compared to Lakoff's findings concerning the types of sentence negation, negation as a 'higher verb', and negation-transport or neg-raising transformations of different sorts. Finally, some uses and limits to the applications of logic to semantic description are outlined.[5]

II. A stratificational view: deictic function

As remarked above, there have been numerous and widely differing attempts at explaining the discrepancy between (6) and (7). Introductory grammars tell us that 'the object of a negated verb is regularly in the genitive case' (Cornyn 1959:51, cf. also Stilman, Stilman & Harkins 1972: 246 and Rudy, Youhn & Nebel 1970:54). But we are left wondering what precisely is the difference in meaning between (6) and (7), and what precisely is their relative distribution, i.e. when each is appropriate, permissible, or completely ungrammatical'.

But there is general agreement that the accusative is the marked possibility for negated sentences. This implies that the accusative signals some information that the genitive does not. W. A. Morison (1959:23)

and 1964:292-97) attempts to explain this by the relationship between logical stress and grammatical form (cf. also Ward 1965:211-20). However, the insufficiency of this explanation is demonstrated by examples (8) and (9). The addition of *nikakój* 'no kind of ... at all' to (6) gives (8).

(8) Iván ne čitáet nikakój knígi. 'John not read no kind of book(gen)'

This is surely an addition of emphasis or logical stress. But Morison's hypothesis should predict at least the possibility of an accusative object with this modifier. However, adding the accusative *nikakúju* to (7) produces (9), which is completely unacceptable. Thus, logical stress alone

(9) Iván ne čitáet nikakúju knígu. 'John not read no kind of book(acc)'

does not suffice as an explanation.

Davison (1967:35-42), though recognizing the insufficiencies in Morison's theory, finds it an attractive hypothesis. He therefore undertakes to codify the various possibilities via an extensive system of semantic (and sometimes syntactic) subcategorizations (Davison 1967:42-60). From this analysis he draws a list of generalizations, each covering part of the problem, with stated hierarchies of application, should there be conflicts (Davison 1967:60-63).

However, the list is unwieldy and complex. Moreover Davison (1967: 63) admits of the probable existence of a group of constructions for which the listed criteria do not suffice. But native speakers of Russian have no difficulty with this grammatical area, even at a relatively early age. I suspect research in acquisition would show that young Russian speakers can accurately predict usage in constructions they had never before heard, long before they could be expected to have come up with an analogue to Davison's list.

The ease with which Russian speakers use these constructions suggests that there must be a straight-forward explanation of the phenomenon which predicts all the possibilities without reference to extensive subcategorization. Subcategorization is also generally undesirable as a method of analysis because it is usually completely ad hoc. It is a special case of Plato's dichotomies in definition, as presented in *Cratylus*. In general, the utility of any particular linguistic analysis depends to a great degree on the extent to which its parameters may be applied elsewhere, as the existence of wide-spread application enhances the probability of validity in any given instance. This does not concern so-called language universals, for most such claimed or attempted universals are in reality model specific and not provably better for the description of any given language than many other functives. It concerns parameters which can be used to explain other phenomena of the language in question. Utility in other languages is desirable and perhaps even mildly supportive, but by no means necessary.

In short, we must seek out a simple, straight-forward explanation, one which is justified by the structure of Russian and which is, above all, logically consistent with the other facts of the language. It will, preferably, explain other phenomena in the language, and may possibly be the basis of valid solutions to problems of other languages, though this interlingual feature is the least important of the criteria discussed.

In the case at hand, I propose to predict the occurrence of accusative objects with negated verbs by the semantic feature 'deixis'. In general, a word or a part of a word may have deictic function. This implies a specific referent, either linguistic or non-linguistic, which is out of the immediate context and to which it points. In English the expressions 'the book', 'this book', and 'that book' all have specific referents because 'the', 'this', and 'that' are deictics. Conversely, the expression 'a book', where 'a' is not equivalent to 'one', does not by itself have deictic function, and cannot have a specific referent which is not provided by context. In the expression 'there's a book on the table', the specificity is given by context and the juxtaposition of expressions. No single word or part of a word by itself requires a specific referent for 'book'.

Now accusative is the less common in negated sentences and sometimes exhibits logical emphasis, which is a possible consequence of deixis. This agrees with the treatment of accusative as the marked alternative.

There is one caveat, however. The concept of markedness used herein is Praguian. In semantic terms, the marked alternative has a more precise, more closely bound meaning. Consequently, it is appropriate only in certain restricted situations. But by the same token, it will convey more and more accurate information. Conversely, the unmarked alternative alternative is less precise and has a less rigidly bound meaning. It also conveys less information, whatever its context.

As a general rule, the unmarked alternative, because of its relative haziness of meaning, is appropriate to many more situations than the marked alternative. Because of its precision, the marked alternative has a restricted domain of application. In semantics, as in phonology, if two alternatives preempt the entire spectrum of possibilities, it is the unmarked which is found in the position of neutralization, i.e. those contexts in which only one possibility exists. Moreover, the unmarked alternative can substitute for the marked any time the speaker wishes to convey less information or simply not be pinned down. I call this the principle of linguistic slop.

For an example of a clear marked-unmarked relationship in Russian, consider the system of verb aspect: perfective verbs are marked with respect to their imperfective mates. If his father is away from the house, a child may say *otéc ušël* and inform the listener that 1) his father went out and 2) he is presently gone. Or the child could simply say *otéc uxodíl*, which would only inform the listener that his father went out at some time in the past. The latter is less precise and does not convey all the infor-

mation the former does. In particular, it does not say whether the father has returned. If the father went out, came back, and is now sitting in the house, out of sight, the child could still say *otéc uxodíl*, which would be true if uninformative, but he could not truthfully say *otéc ušël*. The former would be 'the truth ... and nothing but the truth,' but it would not be 'the whole truth'. This is, after all, the child's privilege. Family business is not, in general, subject to public scrutiny. But the latter statement would be a lie, as it would imply something contrary to the actual situation.

Return now to a consideration of the hypothesis that the accusative case communicates deictic function in the presence of a negated verb. One way to test this hypothesis is to add a deictic modifier to the noun phrase which constitutes the direct object and see what happens if the verb is negated. In English, as mentioned above, the most common deictic modifiers are the definite article and the demonstrative adjectives. The distinction between the definite and indefinite articles does not exist in Russian. But Russian does have two demonstratives: *ètot* and *tot*, both meaning 'this, that'. The only difference between them is that *ètot* indicates an object near(er) the speaker.

In any case, the modification of (5) is given in (10), and its negation is in (11). Deixis is expressed in both sentences by the demon-

(10) Ivǎn čitǎet ètu knígu. 'John read this book(acc)'

(11) Ivǎn ne čitǎet ètu knígu. 'John not read this book(acc)'

strative. Sentence (11), with a negated verb and deixis in the object, has an accusative instead of a genitive, as expected.

Conversely, sentence (8) has *nikakój* 'no kind of ... at all' modifying the object. This adjective, though giving logical emphasis, seems the very opposite of a deictic: it not only does not communicate any particular referent, it denies the very existence of a particular referent. That is, it allows no referent at all, neither specific nor generic, linguistic nor non-linguistic. Characteristically, it also comes out in the genitive, and the version of this sentence with an accusative object is ungrammatical (cf. (9)).

Now the principle of linguistic slop would predict that, though an accusative cannot be substituted for the genitive of (8) without loss of grammaticality, the opposite is permissible. A genitive can be substituted for the accusative in (11). Sentence (12) is perfectly grammatical.

(12) Ivǎn ne čitǎet ètoj knígi. 'John not read this book(gen)'

In fact, it seems that sentences like (12) are more common than (11). The latter type is considered bookish or hyper-correct. But sentence (12) has the unmarked genitive case used for the object of the negated verb in place of the marked (for deixis) accusative. Sentence

(12) is still marked for deixis by the presence of the demonstrative adjective. This implies that (11), with the demonstrative and the accusative object, is doubly or redundantly marked for deixis. But all communication systems have a great deal of redundancy built into them, and the Russian language is no exception. One need only note the double negative in (8) for an additional and obvious example. The gradual obsolescence of (11) in defe ence to (12) simply exhibits the elimination of redundancy.

Nicholas Alssen has subbested (personal communication) that in the environment of perfective verbs, the genitive of negation is almost never found, though the contrast is always possible with imperfective verbs. Rather than adding a problem, this gives additional support for the hypothesis. Perfective verbs are the marked members of this opposition. Without a detailed description (cf. the short discussion above and Forsyth 1972), it is still possible to state that perfective verbs communicate a very specific, particular act. As such, their effects should be specific, i.e. the patients of such acts should be specific. Thus deixis should be present.

Table 4 summarizes the above findings. Note that (11) has two mark-

		Negation	Acc Deixis	Demonstr Deixis
(5)	Iván čitáet knígu			
(6)	Iván ne čitáet knígi	+		
(7)	Iván ne čitáet knígu	+	+	
(8)	Iván ne čitáet nikakój knígi	+		
(10)	Iván čitáet ètu knígu			+
(11)	Iván ne čitáet ètu knígu	+	+	+
(12)	Iván ne čitáet ètoj knígi	+		+

Table 4

ings for deixis, as mentioned above. Now (5) and (6) are completely unmarked for deixis. The speaker may have a specific book in mind, but we cannot deduce this from either sentence. Sentence (5) by itself cannot communicate deixis. This can only be done by a direct modification of the sentence, as in (10), or by the context supplied by another sentence. Similar comments hold for (6). But the speaker who says (7), (10), (11), or (12) does communicate deixis, and the natural assumption

is that he does have a specific book in mind. If he does not have a specific book in mind, he is misleading the listener.

(8), on the other hand, cannot communicate deictic function, because of the semantic properties of *nikakoj*. If he attempts this sentence with the accusative case, he runs into difficulty precisely because the accusative normally signals deixis in the environment of a negated verb. With *nikak-* signalling 'anti-deixis' and the accusative signalling 'deixis', the communication system would have a paradoxical situation, a situation which could be resolved only arbitrarily. Since arbitrarity cannot communicate anything, this would defeat the whole purpose of the communication system. The Russian language effectively eliminates this threat by treating the paradoxical combination of *nikak-* with accusative as ungrammatical (cf. (9)). There is no syntactic reason why this should be so, but it is precisely the resolution of the paradox predicted by markedness theory in terms of the semantic markings. The explanation of the 'ungrammaticality' must therefore be found in the (linguistic) semantics of Russian.

Deictic function is also an attractive explanation because it provides insights into other problems in Russian grammar. For example, it explains the contrast between expressions with the normal genitive and otherwise identical expressions with the second genitive (*tarelka supu/supa* 'a/the bowl of soup/), is present in the 'meaning' of *tot*, etc. In addition, deixis seems to be not only language universal, but communication systems universal.

With its underlying logical validity, with such extended usefulness in Russian grammar, and with its universality in communication syste , the concept of deixis is a good, natural explanation of the problem considered herein. Moreover, it is a description of the phenomenon in Russian grammar which is both palatable to and easily assimilated by students. It allows them to interpret Russian and to produce good Russian sentences of this type by making one straight-forward choice. Lastly, it is a description of the sort which parallels the intuitive ease with which Russians seem to operate in this area. Such descriptions are necessary and even crucial if foreigners are to be taught Russian that is more than just acceptable.

Now the simplicity and accuracy of this description do make it effective for both teaching and explanation. But it blithely ignores a very important question: the logical function and scope (and hence 'meaning') of negation. This question is studied in section V below.

However, before treating the logic of clause negation and its effect on the direct object in Russian stratificationally, I must provide a view of clause (sentence) negation as treated by several transformationalists, including researchers in both 'extended standard theory' and generative semantics frameworks.

III. A digression

Since Klima 1964 there have been many studies in the generative mode concerning negation in English. Some of these, e.g. Jackendoff 1969 and 1971, have been in the lexicalist or 'extended standard theory' framework; others, e.g. G. Lakoff 1970b, R. Lakoff 1969, and the relevant portions of G. Lakoff 1970a, have been in the generative semantics framework. Two continuing and not unrelated arg ments have concerned the evidence favoring a hypothesis which treats quantifiers as higher verbs (cf. Carden 1967, Lakoff 1970b, and Jackendoff 1971) and the occurrence of the Not-transport or Neg-raising transformation (cf. R. Lakoff 1969, G. Lakoff 1970b, Lindholm 1969, Jackendoff 1971, and Chapin 1973). It is my feeling that the treatment of quantifiers as higher verbs, though a laudable attempt to account for a long-troublesome phenomenon, tends to deprecate the logical nature of quantification and treat it as just one more syntactic level. In addition, Jackendoff (1971:282-87) has shown that the syntactic arguments supporting the hypothesis of quantifiers as higher verbs are at best inconclusive. Concerning Neg-raising, Jackendoff (1971:291-96) also correctly points out that the syntactic arguments favoring such a rule are shaky, too, though he fails to convince me that Klima 1964 has the correct solution, and not only because my distribution of asterisks is radically different from both Klima's and Jackendoff's. Conversely, Lakoff (1970b:160) argues against the lexicalist (interpretive semantic) description by pointing out that sentences like (13), with

(13) Joe doesn't believe that Mary expects Bill to lift a finger.

so-called negatively-favored elements as the final infinitive phrase, require a negative environment for that choice. Lakoff's solution is that such an item can be chosen only in sentences containing negation in deep structure. The deep structure of (13) would have several embeddings: (Joe believes (Mary expects (Neg (Bill helps)))). In the environment of *neg* in the deepest sentence, 'lift a finger' can be chosen as the realization of 'help'. Then (13) is derived by cyclic application of Neg-raising. Chapin (1973:2-3) points out the difficulty in this, i.e. that all intervening verbs must be members of the restricted class which admit Neg-raising. He argues that there are verbs with main-clause negation which do not admit Neg-raising, but which choose the negatively-favored element in the subordinate clause, e.g. (14) and (15).

(14) Joe didn't force Mary to pay a red cent.

(15) Mary isn't about to lift a finger.

The only other solution, according to Lakoff, is to allow the to occur indefinitely far from the negatively-favored element whose choice is governed by it. This he (and Chapin) find unacceptable because, as Lakoff (1970b:160) remarks, 'All known selectional restrictions on the occurrence of lexical items are finite in scope.' Surely, this is both an accurate observation and a psychologically desirable constraint for a grammar which does not have the mechanism to look beyond a single S. Moreover, it seems consistent with the practical restrictions on short-term memory.

But Chapin (1973:3) claims that the solution to this problem is the creation of a class of quasi-modal predicates which 'differ from true predicates in being potential clause mates in underlying structure.' Thus (14) and (15) are grammatical because there are no clause boundaries between negation and negatively-favored element. He concludes by admitting that his solution is incomplete (Chapin 1973:8).

I will not attack any of Chapin's arguments. However, I would like to suggest that, though some such categorization of predicates may be necessary, it is not the simplest or most direct solution to the problem.

First, consider Lakoff's statement of the alternative to cyclic application of Neg-raising. This alternative is to require 'that a negative be present somewhere, perhaps indefinitely far away ... (requiring) selectional restriction to extend over an indefinitely large stretch of tree' (Lakoff 1970b:160). The weakness here, it seems to me, is concerned with erroneously substituting linear order for hierarchical dominance on the semantic level.

For example, several people I asked rejected (13) out of hand, substituting (16). Further questioning showed that they were applying the

(16) Joe doesn't believe that Mary expects Bill to help.

scope of the negation to the verb 'believe' alone. This seems to indicate the questionable (one might almost say 'unnatural') character of first applying a negation to one verb, then yanking it several sentences away. This is especially true if the motivation for such a procedure is only to explain sentences like (13). Moreover, sentences like (16) could be used to develop loopholes in Chapin's description.

The solution I would suggest is that the scope of a particular occurrence of negation can be specific (any act or attribute) or general over an entire predicate relation. If its scope is that verb which can be replaced by a negatively-favored element, the conditions are fulfilled, as in (17), which can have the first negation or not. Note that this

(17) Joe (doesn't) believe(s) that Mary expects Bill not to lift a finger.

also accounts for several independently occurring choices of negation. If the logical scope of the negation is the entire proposition, the environment for choice of negatively-favored element is also fulfilled, since all acts in the proposition are somehow negated, and we get (13). We need only add a simple syntactic (surface structure) constraint that the negative element be affixed to the first (or only) verb within its scope. This is a special case of a very general constraint on tactic occurrence for all levels of linguistic structure, to wit: if an element on a given level of structure may potentially be realized in several positions in a particular chain, its natural or unmarked realization is in the first possible position that allows fulfillment of all obligatory tactic requirements.

On the other hand, if the scope of the negation is just the act 'believe', we would get (16) and not (13), since conditions for the choice of 'lift a finger' would not be met. In this way, the problem is solved without recourse to a moot, cyclically-applied transformation like Neg-raising or the creation of a new subcategorization of predicates like the quasi-modals.

Though section V below concentrates on simple transitive clauses, Russian has a problem parallel to the English problem. It concerns the occurrence of the (negatively-favored) genitive direct object of an apparently non-negated infinitive which is itself the complement of a negated verb. An example is given in (18). My solution is a simple exten-

(18) Ne veleno raboty delat'. 'not ordered work(gen) to do'

sion of the methods used in section V and parallels the one suggested above (cf. Sullivan in press c). Implicit in it is my approach to the treatment of logical considerations in linguistic analysis, i.e. that they are not purely syntactic. But I return to this point below.

To be fair, I must point out that Lakoff is said to have changed his mind about the description in Lakoff 1970b for several reasons. First, sentence negation is treated, like quantifiers, as a higher verb. This requires, in the normal case, a Neg-lowering transformation. The question this raises concerns the interaction of Neg-raising and Neg-lowering, which is undefined. Second, his solution seems to claim (falsely) that items like 'lift a finger' are independent in language. I must interject that this is not a necessary conclusion of his original solution, only a possible one which can be avoided, though probably not within the generative semantic framework. Finally, the syntactic justification of Neg-raising has been shown invalid. I do not know what he suggests as a solution now.

But note that these objections to Lakoff's solution are model specific. That is to say, they concern violations of the structure of the transformational-generative model. Absolutely no thought is given to the possibility that the solution might violate the structure of language. For example, no consideration is given to the fact that language seems to be able to communicate two types of clause negation.

However, this evidence is presented in proper sequence below. Let us therefore return to the question of negated Russian clauses and consider them from a logical point of view.

IV. Preliminary considerations on the logic and syntax of clause negation

In general, there are three things about clause negation which any description must account for. First, the syntactic facts must be described. Second, the 'meaning(s)' of the negated construction and its opposition to the meaning(s) of the non-negated constructions must be explained. Third,

the relationships between the set of possible meanings and the set of possible syntactic constructions must be described. An integration of these three should permit insights into the ways language communicates what it can communicate concerning negation. That is, we must if possible account both for the way a negated concept is encoded syntactically and for the way a negated concept is decoded or deduced from a syntactic construction.

This leads to one further consideration. Logicians have long been aware that negated quantified propositions can differ as to the locus and scope of the negation (Copi 1967:168). If such a datum is linguistically significant, then it must pertain to a level of language between syntax and referential meaning, a level on which logical and more general semiotic functions are specified. We must therefore test for possible different scopes of negation and consider the structure of the portion of language needed to describe them.

Consider the simplest possible sentence type in Russian, outside of interjectional 'clauses' using imperative verbs or vocative nouns. It involves some kind of 'be' predication as, for example, (19). The verb

(19) kníga zdés' 'book (is) here'

'to be' lacks a present tense in Russian, other than a few relict forms. Except for the negated *net* 'it is not, no', they seem never to be obligatory. The symbolization of (19) is given in (20).

(20) $(\exists x)[Kx \ \& \ Zx]$ 'there is something which is a book and which is here'

The logical possibilities for negation in a conjoined proposition include the negation of any part. The members of the set are enumerated in (21) through (24).

(21) $(\exists x)[Kx \ \& \ {\sim}Zx]$ 'there is something which is a book and which is not here'

(22) $(\exists x)[{\sim}Kx \ \& \ Zx]$ 'there is something which is not a book and which is here'

(23) $(\exists x){\sim}[Kx \ \& \ Zx]$ 'there is something which is not both a book and here', i.e. 'there is something which is not a book or is not here'

(24) ${\sim}(\exists x)[Kx \ \& \ Zx]$ 'there is nothing which is a book and is here'

The Russian equivalents to (21) and (22) are given in (25) and (26).

(25) kníga ne zdés' 'book (is) not here'

(26) *ne kníga zdés' '?non-book (is) here'

(25) is perfectly straight-forward, but (26) is concocted. An expanded version of (26), given in (27), is acceptable but irrelevant. (27) can-

(27) zdés' ne kníga a gazéta 'here (is) not a book but a newspaper'

not be a negation of (19).

I am at a loss to translate (23) directly. But by the rules of negative universal quantifier introduction/elimination, (23) can be proved equivalent to (28), which is translated by (29). (29) is also perfectly

(28) ~(x)[Kx & Zx] 'not all things which are books are here'

(29) ne vsé knígi zdés' 'not all books are here'

acceptable, but it cannot be a negation of (19).

As a Russian translation of (24), (30) is proposed. The English

(30) zdés' nét knígi 'here not book(gen) = no book is here'

interpretations of (24) and (30) are not identical, as are the interpretations of (21) and (25). But there is some substantive relation here. Consider first the intuitions of native speakers that (30) somehow denies or negates the existence of a book at some space-time locus. Consider next that for (24) the entire proposition is negated, but no single element of the proposition is negated, as in (21) and (25). Moreover, in (30) a negative element *nét* is introduced, and what is the nominative in (19) and (25) is in the genitive case. Consider also that the adverb *zdes'*, which operates alone as the predicate of (19) and (25), is only part of the predicate in (30).

Finally, there is the question of the difference between a simple assertion and an assertion of existence, i.e. between 'a book is here' and 'there is a book here'. The negated parallels would be 'no book is here' and 'there is no book here'. Generally logicians do not distinguish between the two types in practice (cf. Copi 1967:168), though Fitch (personal communication) does for the negated pair by differing scope of quantifier negation. However, language can and does distinguish between the two types for any assertion. Thus (30) and its alternate word-order agnate sentences, e.g. *knígi zdés' nét*, probably represent the neutralization of different semantic structures in different environments. The distinction seems to be everywhere overt in English, but it is often implicit in Russian. This is a very general problem which overlaps with the problem of negation. Its description must eventually be integrated with that of negation, but it is beyond the scope of this study. However, the important thing is that (30) is not necessarily just an existential sentence.

In any case, though (30) and (24) are not isomorphic, they do seem to share a common interpretation. It may therefore be concluded that (25) and (30) are the two possible negations of (19). Since all logical possibilities have been considered, no other negations of (19) are to be found.

Now the syntactic differences between (25) and (30) are manifold, as detailed above. But if both can be negations of (19), they must be directly related to each other and to (19). The differences between them preclude any overt syntactic relations, and asserting a relationship on some non-linguistic level of referential meaning would be begging the issue. However, compare the symbolizations given in (20), (21), and (24). These are expressive of some level between the linguistic (syntactic) and the non-linguistic cognitive or referential system. (21) and (24) each differ from (20) only by the occurrence of negation, i.e. in a minimal semantic (sememic) way. (24) differs from (21) only by the scope or logical dominance of the negation. (21) has act-specific negation and (24) has quantifier negation. Thus a semological structure which specifies hierarchical ordering (logical dominance) and other semiotic functions is a necessary part of linguistic structure. A separate but related syntactic structure is also necessary, because hierarchical dominance and linear ordering must be expressed separately, as is shown below. Thus the logic of clause negation in natural language parallels purely logical considerations given by Copi (1967:165-68).

In sum, then, three hypotheses concerning negation and its effect can be formulated.

> (H1) Either the act or the quantifier may be negated (i.e. all negation may not be ascribed to one kind of 'sentence negation).
>
> (H2) If the act alone is negated, then a) the semantic scope of the negation is the verb alone and b) the syntactic realization of negation is simply the proclisis of *ne* to the negated predicate.
>
> (H3) If the quantifier is negated, then a) the semantic scope of the negation is the entire proposition and b) the syntactic realization of negation includes proclisis of *ne* to the verb (or the appearance of *net*) and the choice of negatively-favored genitive case ending.

These hypotheses and their consequences are tested on simple transitive sentences in section V below.

V. Another stratificational view: the logic of clause negation

Consider again the basic subject-verb-(accusative) direct object sentence, given as (5) above and repeated as (31) here.

(31) Ivǎn čitáet knígu. 'John read book(acc)'

Remember that Russian has no way of expressing simple deixis, which is signaled by 'the' in English. There is thus no way of telling whether Ivan is reading 'a' book or 'the'book, and (31) must remain ambiguous vis-a-vis the specificity of the object. Only greater discourse block context can clarify the issue. The symbolic representation of (31) is given in (32).

(32) $(\exists x)[Kx \ \& \ \check{C}ix]$ 'there is something which is a book and which Ivan is reading'

Simple deixis can be represented in mathematical logic. A hypothetical Russian equivalent to 'Ivan is reading the book' would be symbolized as in (33). There are other possible symbolizations (cf. Copi 1967:164-

(33) $(\exists x)[(y)[Ky \equiv [x = y]] \ \& \ \check{C}ix]$

68), but (33) is the simplest. The logical representations of simple deixis are known as the 'iota function'. Now deixis is a concept relatively young children have little difficulty with. By age 4 both my older children had a solid grasp of it. Given this fact, it seems that deixis should be a simple, straight-forward sign. It is reasonable to assume that the expression of deixis is responsible for the difference between (32) and (33). But (33) is hardly simple, and even after a seme ter of logic, students do not find this type of symbolization obvious. To the contrary, I would find any claim that very young children must have internalized a generalized iota function absurd. Some other approach must be found. But I return to this point in section VI below.

Consider the possibilities for negation in (32). What can be negated in (32), according to (H1), is the act or the quantifier. These possibilities are given in (34) and (35), respectively. Note that what is

(34) $(\exists x)[Kx \ \& \ \sim\check{C}ix]$ 'there is something which is a book and which Ivan is not reading'

(35) $\sim(\exists x)[Kx \ \& \ \check{C}ix]$ 'there is nothing which is a book and which Ivan is reading'

negated in (34) is not the 'verb' but the logical relation between 'i' and 'x'. The 'verb' is a syntactic category and has never been shown to be appropriate to the semantic level. The fact that the act is normally realized as a verb in syntax is irrelevant. Moreover, there is no way

of negating Ivan (i) or the book (x) alone within the predicate relation which symbolizes the act. Negation is not isolated.

There are two negated sentences corresponding to (31), and these are given in (36) and (37). To repeat: the genitive direct object in

(36) Ivǎn ne čitáet knígu. 'John not read book(acc)'

(37) Ivǎn ne čitáet knígi. 'John not read book(gen)'

(37) is more frequent, but the accusative, as in (36), also occurs. I argue in section II above that the accusative object in (36) represents marked simple deixis. This suggests a parallel between (34) and (36), and between (35) and (37). Now (35) is certainly a categorical negation, and (37) can be categorical in its usage. A more emphatic categorical negation is given in (38), which must have a genitive direct object (cf.

(38) Ivǎn ne čitáet nikakój knígi. 'John not read no kind of book at all'

(8) above).

The assertion in (34) that 'there is some book' allows the specific referent necessary for deixis to be operative. Moreover, in (34), nothing is negated (or denied) about the book or its existence. Only a particular relationship between Ivan and the book is denied. Thus the structural deixis marked syntactically in (36) and the logical deixis possible in (34) provide a dual approach to the referential meaning expressed. I return to this in the integrated description given in section VI below.

It is now clear that (H1), which predicts a difference between the negation of the predicate relation and the negation of the quantifier, has been borne out by the distinction between (34) and (36), (35) and (37). If the logical negation is act specific, the semantic scope of the negative is the relation and not just its object. Deixis with reference to the object is present. If the quantifier is negated, the semantic scope of the negative is the entire proposition. Negation is generic, including the object and the act relation, and no deixis is expressed. The syntactic realization of negation in either case is the proclisis of *ne* to the verb. But if the quantifier is negated, the negatively-favored genitive is chosen for the direct object. Thus (H2) and (H3) are both validated for simple transitive sentences.

VI. A third stratificational view: an integrated description

The juxtaposition between the two descriptions shows a serious discrepancy. The deictic solution requires marked accusative, and the logic of negation s lution requires marked genitive. Moreover, the former ignores the logic of negation, and the latter does not account for deictic function explicitly.

As remarked above, native speakers have found both descriptions plausible. I assume that this is significant, and conclude that some way of combining the two descriptions must be found. The problem in any integration of disparate descriptions is a starting point. That an integrated description is at all possible indicates that the discrepancy between the descriptions is an anomaly rather than a contradiction. It is possible that the anomaly arose as a consequence of the one-sidedness of the two descriptions, and an integrated description would resolve it.

The primary assumption, therefore, is that the basic conclusions of both descriptions are valid. Specifically, 1) the accusative case is a realization of marked simple deixis in the environment of a negated verb; 2) negation can be categorical over the entire proposition or act-specific; and 3) categorical negation requires a syntactic marking of genitive with the direct object. In addition, we should account for the fact that if a demonstrative is present, both genitive and accusative are understood as communicating deixis, but that the genitive is now preferred and more frequent. However, this last datum involves competing structures, as a change is in progress.

The data to be accounted for in the integrated description are summarized in table 5 (cf. table 4). The plus mark indicates definite presence, the minus mark indicates absence, and the question mark means that it is impossible to tell without greater discourse context.

		Acc Deixis	Demonstr Deixis	Act Neg	Categorical Negation
(5)	Iván čitáet knígu	?	-	-	-
(6)	Iván ne čitáet knígi	-	-	?	+
(7)	Iván ne čitáet knígu	+	-	+	-
(8)	Iván ne čitáet nikakój knígi	-	-	?	+
(10)	Iván čitáet ètu knígu	?	+	-	-
(11)	Iván ne čitáet ètu knígu	+	+	+	-
(12)	Iván ne čitáet ètoj knígi	-	+	+	-

Table 5

Note that marked deixis and categorical negation are in complementary distribution. Conversely, act-specific negation might not require the expression of deixis. We might get (6) in contexts in which it is plain that a particular book is in question. But this fact would be communicated by

the context alone. Admittedly, (7), (11), or (12) would be more precise and probably more likely in such a context, but they would not be obligatory. It is remarked above that there is a change in progress from a situation in which (12) was not encoded to a situation in which (11) will not be encoded but will be decodable. Since the situation is presently in flux, a simplifying strategy is proposed. As a first approximation, ignore the combination of genitive of negation with the demonstrative as in (12). This is an admittedly obsolete state. A possible future stage which signifies the end of the change is described below (cf. fn. 6).

There are thus six facts to be accounted for in the specification of complement case:

a) the normal (or least marked) case marking is accusative, which occurs in (5);

b) the normal case marking under negation is genitive;

c) negation may be act-specific or categorical;

d) simple deixis requires accusative in negated clauses;

e) categorical negation requires genitive; and

f) categorical negation and deixis cannot be expressed simultaneously; deixis takes precedence.

These are not six independent bits of data; they are interrelated. (a) and (b) refer to the lexotactics, (c) and (f) to the semotactics, and (d) and (e) to semolexemic relations. That is, (d) and (e) are verbal expressions of realization rules or realizational relationships. More specifically, the combination of (a) and (b) requires a lexotactic relationship as expressed in (39) and figure 4.

(39) Clause Complement/((neg ⊃ gen) + acc)·Pa

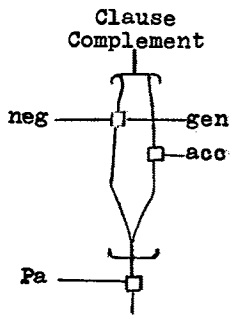

Figure 4

This relationship states that, when patient (Pa) is realized in clause complement position, the accusative case lexeme is provided except when negation is present. In the environment of negation, the preferred left-hand path is taken and genitive case marking is provided.

The only lexotactic question remaining is whether the deictic accusative must be overtly marked in addition. In the obsolescing variety of Russian complement structure assumed here, the additional marking of accusative would be an unnecessary com-

plication. The genitive is a realization of categorical negation. That is, it is an occurrence of negation which dominates the act-patient relation in the semotactics. If simple deixis is described as taking precedence over categorical negation, and hence, blocks it, the semotactic conditions which evoke a marked genitive in the lexotactics. With no occurrence of genitive, the lexotactic structure of figure 4 automatically provides an occurrence of the accusative case lexeme. At the same time, the semotactic provides an occurrence of act-specific negation, which is realized simply by the proclisis of *ne* to the verb. This takes advantage of redundancies in semolexemic interrelations and results in a simpler description.

But in addition to simplicity, there are 'structural' semantic reasons for preferring such a description. First, not overtly marking a 'deictic' accusative but suspending the categorically-determined genitive ensures that the only overt mark of deixis below the sememic stratum is a demonstrative. (There are other deictic modifiers. But they add confirmatory information to this limited sample.) The deictic accusative in (7) is implicit, and in (11), redundant. It is communicated by contrast with the genitives in (6) and (12). Moreover, if no deixis is to be communicated, negation will fit into categorical position in the semotactics, causing a lexotactic specification of the genitive case. This results from the fact that a relational-network (gestalt) structure realizes an -eme on any stratum in the first available position. This also predicts the statistical dominance of the genitive in negated clauses. If categorical negation is automatic in the absence of deixis, the genitive will be the normal case of the direct object of negated clauses. Of course, other considerations which are beyond the scope of this study may affect this description. For example, contrastive sentence stress (marked information focus) might effect contexts without deixis which require act-specific negation and hence accusative direct object. But phenomena of this sort are easily integrated into the description, and they express an opposite tendency rather than a contradiction. Ignoring these possibilities, moreover, is probably not misleading.

Further, the description has other structural semantic properties. It resolves the question of whether genitive or accusative is more highly marked in negated clauses; this is the question which had the anomalous answers in the two above descriptions. Figure 5 demonstrates that the circumstances resulting in an accusative in a negated sentence are the most highly marked, but that they do not directly result in an accusative. Only a genitive of categorical negation is marked directly. In addition, simple deixis which is marked explicitly only in the semotactics, and which is realized implicitly in the lexotactics, predicts a tendency to lose the ability to mark simple deixis and to require that deixis be signalled only in deictic quantifiers, whether they are realized adjectivally or pronominally in the lexotactics. Finally, as remarked above, the latter description (of the two originally mentioned) is both simpler and more straight-forward.

But effecting the semotactic description requires some explicit, straight-forward, and above all simple statement of deixis. Fitch (in press) has suggested that the definite article and the other deictic

modifiers constitute a set of quantifiers in natural logic. They are not elaborate modifications of the existential quantifier in combination with the universal quantifier but entities in their own right. I find his arguments compelling, and their consequences believable. It seems clear to me that mathematical logic is not and can not be sufficient for describing the logical hierarchies in the sememic structure of natural language. Instead, a natural logic must be defined. Thus the existential quantifier used above is replaced by a deictic quantifier *t* (to be read 'the x') in the algebraic formulae below.

(40) presents the semotactic relations of the set of sentences given in table 5, excepting only (8) and (12). The relations between

(40) Predication/(Ivan·$\underline{Ag}$)·([t + (~·$\underline{C}$)]·(([~]·čita)·(knig·$\underline{Pa}$)))

the sememes *Ivan*, *Ag*, *t*, ~, *C*, *čita*, *knig*, and *Pa* is conjunctive or the 'and' relations. The sememes *Ag* 'agent' and *Pa* 'patient' are roles (or in Fillmore's framework 'cases') supplied to *Ivan* and *knig*, respectively, as markers of their sememic functions. The sememe *C* marks the semotactic position of categorical negation. It is realized as the lexotactic specification of genitive described in figure 4 above. The sememe of negation (~) as such is realized as *ne*, the verb-proclitic in the lexotactics. The sememes underlined in (40) are semotactically determined elements. They mark positions within the semotactic hierarchy and have themselves no direct relation to meaning.

Another important feature of (40) is the hierarchic dominances expressed. The agent dominates the rest of the predication. Deixis applied to the patient and categorical negation (potentially, since their occurrence is optional) dominate the act-patient relation. Act-specific negation dominates only the act. (for a justification of much of the hierarchic dominance used herein, cf. Sullivan in press a.)

The relevant syntactic relations of the sentences in table 5 are given in (41) through (45).

(41) Clause/Subject Verb [Complement]

(42) Verb/[ne] (cita, ...)

(43) Complement/((C ⊃ gen) + acc)·Pa

(44) (Pa, ...)/NP

(45) NP/(Ivan, knig, ...)

A great deal of information concerning the clause has been omitted. Much of it is important in its own right, but none of it is relevant to the issue at hand. The two crucial points are included in (42) and (43). (42) states that the sememe of negation, whatever its logical domain, is realized as the lexeme *ne*. (43) predicts the occurrence of the genitive direct object if the semotactic domain of negation is the act-patient relation, i.e. if it is categorical.

The full graphic description, including semotactic and lexotactic arrangements and semolexemic realizations, is given in figure 5 on the following page. To trace the encoding process, begin at the top of the semotactic portion. A sememic input is assumed (from 'meaning,' toward the left side of the diagram), which always includes *Ivan*, *čita*, and *knig*. Additionally, there may be deixis, negation or both. What happens during encoding is summarized in table 6. Case 1 shows what happens when the three above sememes occur with negation only, case 2 combines them with both negation and deixis, and case 3 includes neither.

	A	B	C	D	E
1.	~	PredRel	~ C	ne gen	2 4
	Ivan	Event	Ivan Ag	Ivan ø	1 subject position
	čita	Patient	čita	čita	3
	knig	Act	knig Pa	knig ø	4 object position
2.	~	čita	~	ne	2
	t (simple deixis)	PredRel	ø	-	-
	Ivan	Event	Ivan Ag	Ivan ø	1 subject position
	čita	Patient	čita	čita	3
	knig	Act	knig Pa	knig acc	4 object position
3.	Ivan	Event	Ivan Ag	Ivan ø	1 subject position
	čita	Patient	čita	čita	2
	knig	Act	knig Pa	knig acc	3 object position

Table 6

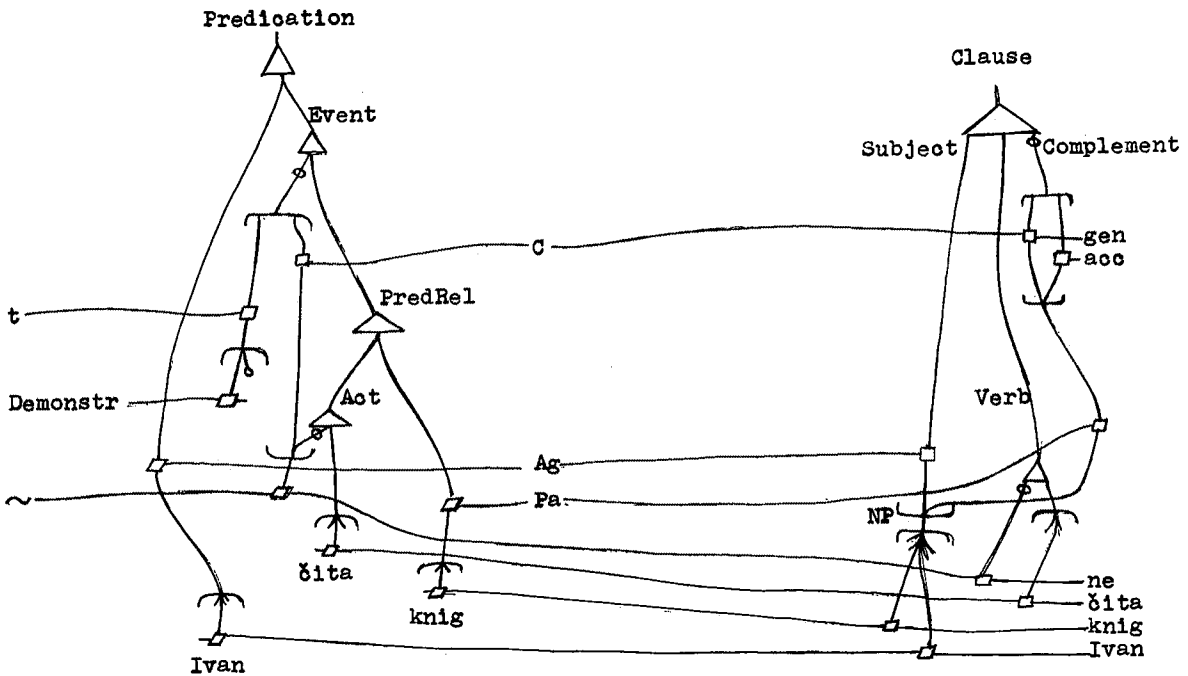
Predication
Event
PredRel
Act
t
Demonstr
~
čita
knig
Ivan
c
Ag
Pa
Clause
Subject
Complement
gen
acc
Verb
NP
ne
čita
knig
Ivan

Figure 5

In column A, the input to the semotactics during encoding is summarized. Column B tells what the sememes in column A dominate hierarchically in the semotactics. In the case of *čita* and *knig*, the act and patient, the actual relationship is one of co-dominance, because they are on the same tactic level and dominate each other. But this is of no significance in the present example. Column C tells the sememes each semotactic element is realized as below the semotactics and column D gives the lexemic realization. Column E tells what lexotactic position, in terms of linear ordering, is assigned to the realizations of the various sememes in each case. Note that some sememes, in this case the agent and patient roles, are realized as lexemes of arrangement.

In all three cases, the agent *Ivan* dominates the event. It is realized as the sememe *Ivan* with a determined occurrence of the sememe *Ag*. The sememe *Ag* is related to subject position in the clause, and this must be the position of realization of the lexeme *Ivan*. In all three cases, *čita* and *knig* (the act and the patient) are codominant. The former is realized as the sememe *čita* which is realized as the lexeme *čita* in verb position in the clause. The latter is realized as the sememe *knig* which is realized as the lexeme *knig*. In this semotactic construction, it is supplied with the determined sememe *Pa* which effects its realization in clause complement position.

The difference in realization in the three cases results from the semotactic arrangement of negation and deixis: their occurrence, combination, or lack. If only negation is present, as in example 1, it is realized as part of the event relation and dominates logically the the entire predicate relation (PredRel). The determined sememe *C* is provided. Then, as the patient is being realized in complement position of the clause in the lexotactics, the encoding path down from complement to the diamond to which *Pa* is connected must travel via the preferred or marked left-hand branch of the ordered 'or' node. This provides a determined occurrence rence of the genitive case lexeme to the direct object. If negation is lacking, the sememe *C* is not supplied, and this left-hand path to *Pa* is not active and cannot be taken. This means that the genitive lexeme is not supplied. Because the left-hand branch cannot be taken, the right-hand branch must be taken and the accusative lexeme is supplied. If both deixis and negation are present in the semotactics, deixis takes precedence over negation in the event relation. The dominance (and therefore the scope) of negation is thus restricted to the act alone. Because it does not dominate the entire predicate relation, i.e. it is specific rather than categorical, there is no occurrence of the sememe *C*. Again, the left-hand branch in the lexotactics cannot be taken and the right-hand branch must be taken. Again accusative and not genitive is supplied.[6]

The integrated description may be summarized very simply in terms of three generalizations: syntactic, semantic, and 'meaning'.

> SEMANTIC: the scope of negation may be categorical (dominating the entire act-patient relation) or, if simple deixis is present, specific (restricted to the act).

MEANING: categorical negation denies the entire predication; specific negation negates only the act.

SYNTACTIC: negation is always realized as *ne* preposed to the verb; the semotactic position of categorical dominance effects genitive direct object.

From the above generalizations it should be clear that the logical and deictic solutions to the problem are not only compatible, but that they cover different sides to the communicative function. Specific referents and scope of negation are both signalled.

The three generalizations show how a semantic stratum intermediate between syntax and meaning permits the statement of the possibilities of dominance and scope of negation and shows how they are interrelated with simple deixis. From this straight-forward semotactic description, the differences between possible meanings of negation are predictable in logically minimal fashion, as is the occurrence of the genitive of negation.

Moreover, this description has implications for the description of aspect in Russian. Recall Alssen's comment that a negated perfective aspect verb almost never shows the genitive of negation. Figure 5 shows that this very interesting piece of confirmatory evidence can be incorporated in the description. A dynamic verb with a specific patient communicates deixis, which requires act-specific negation and blocks the genitive direct object.

Thus the above description not only shows the complementary nature of deictic function and the logic of negation, correctly predicting the relation between different meanings and their syntactic expression, but offers insights into verb aspect in Russian.

VI. Some consequences of the descriptions

The description given in section V concentrates its discussion on the encoding, meaning-to-sound process. But this description, too, is not unidirectional. It operates in the reverse direction, for decoding, as well. The *ne* is decoded as clause negation, but there is a semotactic ambiguity. Moving upward from the sememe *ne* in the semotactics we first reach an upward 'or' node. If genitive of negation is present, the categorical position of negation is chosen. If no genitive is present, the conditions for categorical negation would not hold. But act-specific negation can be chosen, in which case simple deixis is deduced. Ambiguity in the decoding of negation is thus disambiguated at higher semotactic levels with the (indirect, in this case) help of further syntactic information.

This description is therefore unambiguously bidirectional. That is, it works in both directions, for both the encoding and decoding processes. One is led to the conclusion that the semantic/syntactic level of language and linguistic description is not 'directional,' at least not in the sense of the generative semantics/'extended standard theory' debate.

Another interesting point concerns the shift from hierarchic dominance in the semotactics to linear sequencing in the lexotactics. Negation and genitive direct object can be separated by an indefinitely long stretch of clause structure in the lexotactics, so long as the semotactic occurrence of negation dominates the patient. This obviates G. Lakoff's requirement (1970b:160) of 'finite scope'. But note that there is no linear distance between the sememes *ne* and *C* in the semotactics. Both are related to the position of categorical negation.

Differing hierarchic dominances is a prime consideration of all types of logic. Fitch (1952:155, in press; cf. also Sullivan in press a) shows that differing hierarchic dominances may determine differing equivalences or may block some derivations. He also shows (Fitch in press) that these dominances are translated into linear sequencing. This agrees, to a certain extent, with the findings of G. Lakoff 1970a. But it is significant because Fitch operates from a logician's viewpoint. Further support is provided by the interaction between differing hierarchic dominance and linear order studied above. The description therefore de onstrates that purely logical considerations can be of value to and have a place in linguistic description. They are necessary to semotactic structure and semolexemic realization.

However, the inclusion of functions from mathematical logic are severely limited, as the considerations on deixis and the iota function show. Hierarchic dominance and quantification are necessary, but the types of quantification should be determined by natural logic, which is responsive to the needs of natural language, rather than by mathematical logic.

A similar question concerns logically equivalent propositions. Sullivan (in press a) adduces some (admittedly minimal evidence for the semotactic occurrence of both equivalent forms in one case. This is contrary to the suggestion by McCawley (1971 and elsewhere) that logical rules of direct consequence be included in a statement of language competence. I suggest this claim be subjected to the same scrutiny as the claims concerning the iota function. Some rules of direct consequence turn out to be superfluous, since they are implicit in the structure. An example of one such rule (it is actually a theorem in the Fitch system) concerns the commutativity of the 'and' relation. Other rules of direct consequence are hardly basic to linguistic intuition in any language, e.g. negation elimination, possibility elimination, or existential quantifier elimination.

Now the shift between hierarchic dominance and linear ordering indicates a stratal difference between sememic and lexemic (or semantic and syntactic) structures. The formal expression of both strata and their interrelation by logically minimal realizational relationships describe the situation completely. Thus the description presented in figure 5 is simpler than any possible set of transformations. Conditioning environments do not count. They are implicit in the general description of tactic structure at each stratum. Moreover, it fulfills the considerations Chomsky himself has laid down (cf. the discussion below). The success

of the stratificational model in this case suggest that a similar approach should be tested in other cases, e.g. subject 'raising', along with alternate transformational descriptions.

The results may be surprising, and they should be rewarding.

3. Some general observations

The two studies effected above focus on two areas of language: phonology and the relation of linguistic semantics to syntax. The findings in each section have particular significance for each level of language. In sum, section one demonstrates that a biunique level of contrast is compatible with maximal predictability of non-contrastive features in phonology. The level of biunique contrast also permits greater generalization within phonology and results in less dependence on morphological or syntactic information. Section two demonstrates the interdependence between the communicative functions of negation and simple deixis and predicts the occurrence of the accusative or genitive direct object in negated Russian sentences.

In each case, evidence for the formal stratification of language is adduced. The descriptions bear out the general findings of stratificational grammar concerning the shape of a stratal division: two tactic patterns interconnected by realizational relations. The phonology problem shows the phonemic stratum specifying linear ordering within the structure of the syllable and especially the position of phonemic contrast for voice. Considering the relevant realizational relations, we find two on the phonemic st atum: archiphonemic neutralization (potential suspension of contrast) and the realization of the voice morphon as phonemic voice or as zero (the alternation between phonemic voice and nothing). The hypophonemic or phonetic stratum shifts phonemic voice from a position in a chain to a position dominating the rest of the string hierarchically, i.e. realizes it as a cluster prosody.

The negation problem shows the possible hierarchic dominance of negation on the sememic (linguistic semantic) stratum. The lexemic (syntactic) stratum realizes negation in the pre-verb position in the clause. If the negation occupies categorical position in the semotactics, the realization of the patient in clause complement position is supplied with an occurrence of the genitive rather than the accusative. Here the meaning-bearing element is realized as a separate lexeme in one clause position, and the hierarchic dominance, a logical characteristic of semotactic structure, is realized as a lexemic (eventually morphemic) feature in an entirely separate clause position.

Generative (and most more traditional studies) have denied the pos-

sibility of a stratal distinction in either of these areas. Instead, the alternations are described in terms of a base input and a series of rewrite rules. The argument, when considered at all, is that stratification is unnecessary. But consider: the extended standard theory has 1) a deep structure; produces 2) a surface structure; and requires 3) semantic interpretation to relate to 4) meaning. Thus the 'standard' people agree that we need the three levels (meaning, linguistic semantics, and 'surface' syntax) posited in the above study. In addition, to make certain syntactic generalizations, they need a deep structure which, though somewhat semanticized, is supposedly syntactic. (I say nothing about the separate lexicon and subcategorization rules here.) Thus they recognize four levels, even though the distinctions between them are not formally stratal. And these four levels cannot account for either encoding or decoding. Instead, the syntactic portion is 'generative' and the semantic portion is 'interpretive'. That is, the syntax attempts an analogue of encoding and the semantics decodes. Such a model is hardly 'unified' in its structure, in spite of the caveats in Halle 1959 or the claims of Katz & Postal 1964. Worse, the phonology is supposedly 'interpretive' like the semantics. But phonological 'interpretation' turns out to be an analogue of encoding. Thus, though 'generative' phonology is not itself generative, its direction continues that of the syntax. To be fair, the model(s) used by the generative semanticists make substantively different claims. They posit 1) 'remote' structure; 2) pre-lexical syntax or transformations; 3) lexical insertion; and 4) lexical syntax leading to 5) surface structure. Lakoff adds a level of shallow structure, but his data are both questionable and open to alternate interpretations.

The purpose of this lengthy discussion of well-known and rarely questioned 'facts' is to emphasize the 'directional' nature of language in the view of transformational-generative theorists. But it is the basic premisses underlying the model(s) which effect this directionality. Even Chafe and Fillmore, who have significant and often ignored insights of their own to offer have not escaped the trap.

Contrarily, as Lamb has often insisted (most recently in Lamb, to appear 1974), linguistic structure is not directional. It is simply there. More, its usage is bidirectional--encoding and decoding--i.e. the 'performance' is not divorced from the 'competence'. In other words, the structure is not divorced from its use.

Part 2 answers the question of the relevance of purely logical considerations to linguistics and language positively, contrary to the claims of the lexicalists or extended standard theory adherents. However, their applications to language are different from their functions in formal logic. They cannot apply to language as McCawley and others of the generative semantics school have claimed.

It is remarked above that the realizational relations between adjacent strata are logically minimal. That is, they are composed of the basic 'and' and 'or' relations. Thus there can be no discussion between a group proposing a single $T_{neg/cat}$ (categorical negation transformation)

and those insisting that there are two rules, $T_{neg/cat}$ and T_{genDO} (a genitive direct object transformation). The two descriptions account for the same data, but the single transformation is not logically minimal. The description with the two transformations has logically minimal rules. But the T_{genDO} is obligatory and wholly dependent on the $T_{neg/cat}$ which precedes it (cf. the discussion over whether 'Dative movement' and 'To-deletion' are described by one or two transformations). The sememic description shows that genitive in the direct object is dependent on the occurrence of negation in the semotactic position of categorical negation. But the 'categorical' position is not directly connected to syntactic negation. Instead, it is directly related to complement position in the lexotactics. The functions are both present but separate, though the dependency relations are made explicit.

Similarly, the Dative-movement/To-deletion discussion would never arise in stratificational theory. It is a product of the particular transformational model(s) in use. Specifically, it is the linearly-ordered deep structure which must (optionally) be rewritten with the (dependent but obligatory) accompanying deletions.

Chomsky (1970:passim) keeps returning to the point that the model which has the simpler set of basic axioms but which are better elaborated is preferable to the model with a more extensive (and 'more powerful') but less well elaborated basic axiom set. This is precisely what happens here, where the relationships are purely elaborations of the basic types. Conversely, there are no logical constraints on what kind of transformation can be written. Nor is there a universally-accepted 'simplicity' metric by which the complexity of different sets of transformations can be measured. But the realizational relations in figures 5 and 6 are logically minimal (i.e. locally trivial), and the complexity of competing descriptions can be measured in a straight-forward fashion. The different possible types other than the simple ones mentioned above are well-documented. There are basically two: conjunction and disjunction (cf. Lamb 1973, Lockwood 1972, or almost any stratificational article). Others can be derived therefrom.

It is remarked above that this study would be concerned with certain controversies: the directionality of language and process versus arrangement descriptions. In addition, other controversial issues were scrutinized. These include the formal stratification of language and the opposition between realization and mutation (transformation), the relevance of logic to semantic description, the existence of a level of biunique contrast in phonology, and the use of the archiphoneme and the cluster prosody.

In fact, that is a misleading statement. None of these issues is a 'controversy' in stratificational theory. I studied Trubetzkoy's *Principles of Phonology* in great detail only after writing my preliminary phonology of Russian. Only then did I realize that the stratificational model had forced me to make extensive use of the concepts of archiphonemic neutralization and cluster prosodies. The answer to the question of their

relevance to linguistic theory had emerged from the description which was based only on the preliminary assumption of stratificational theory that language is composed of a network of relationships connecting meaning and sound and by its refusal to divorce competence from performance. Most of these controversies in variants of transformational-generative theory are merely conflicting claims which arise because conflicting and often unnecessary premisses have been assumed for the model. Within this context, the controversies cannot be resolved, because the arguments are bound to be skew. Conversely, a completely different point of view can contribute a resolution of the controversy. In these cases, the stratificational point of view shows them up to be the non-issues that they are.

It has been remarked that many of the problems in the structure of transformational-generative theory have arisen because the theoreticians are insufficiently well versed in the logic of theory construction. This has resulted in an inability to distinguish between a theory and a model of that theory or, as Marchand put it, the inability to distinguish between a theory *of* language and a theory *in* language.

Perhaps the healthiest thing Chomsky ever did was to express scepticism toward many of the sacred cows of the neo-Bloomfieldian linguistic establishment. Unfortunately, he has expropriated the imprimatur and now sits in cathedra himself. Generative theory seems now to be the established norm, and everything else must be defined in its terms. I would suggest that we not conclude, as Lehmann (1972) has done, that linguistic theories (actually models) are converging until we test the results and effects of some radically differing views. In this way many of the claims and counter-claims of several herds of sacred cows can be tested and resolved or rejected without resort to some sort of exclusive, one-or-the-other litmus test. Linguists and lintuistics can only benefit.

Footnotes

[1]This conclusion was reached during the course of a series of studies of Welsh phonology undertaken under close faculty supervision by Toby D. Griffen, a student at the University of Florida. The studies were supervised by D. Gary Miller, Bohdan Saciuk, and William Sullivan. Miller and Saciuk are generativists; Sullivan is not.

[2]One piece of data Lamb was lacking concerns the realization of sonants in word-final position following an obstruent cluster. If the obstruent (or cluster) is voiced, the sonant is voiced, as in *dobr* 'kind'; if the obstruent is unvoiced, so is the sonant, e.g. *Petr* 'Peter'.

[3]In sum, my biunique phonology differs from the American structural or Neo-Bloomfieldian phonology in three ways: it incorporates archiphonemic neutralization and non-segmental phonemes; the level of biunique contrast is considerably more abstract; and the mathematical structure of the phonemic level is non-segmentable. I am indebted to Professor Carlton Hodge (Indiana University) for pointing out my failure to make these three points explicit.

[4]The aim of this section is the definition of the effect of deixis on negation. That is, the study seeks to determine the difference between the Russian equivalents of 'John isn't reading a book' and 'John isn't reading the book'. Therefore, no standard English equivalents are given to the Russian examples.

[5]I must thank George and Nadia Sheffer (University of Florida) for their help in determining the grammaticality of the Russian sentences. Special thanks go to Professor Nicholas Alssen of the Houston Manned Space Center for extended discussions on grammaticality and meaning for different Russian sentences and for his suggestions on analysis and description, though I have not always taken his advice.

[6]Without extended comment, I describe the probable post-change structure in figure 6, including only those relationships which are different from the corresponding relationships in figure 5. Remember, this assumes a stage in which (11) and (12) are both understood, but only (12) is spontaneously encoded.

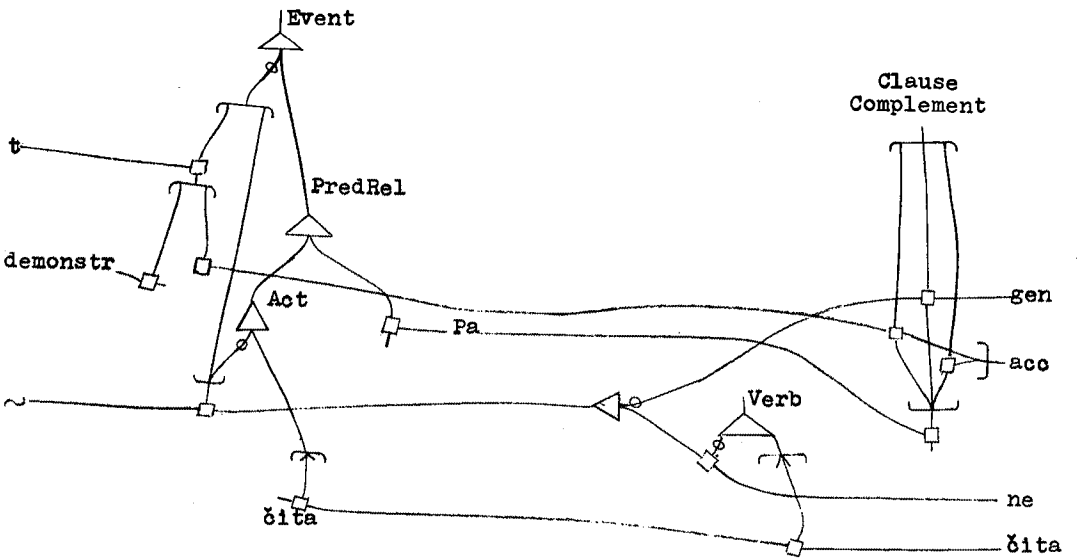
Event
Clause
Complement
t
PredRel
demonstr
Act
gen
Pa
acc
Verb
čita
ne
čita

Figure 6

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On the Pedagogical Applicability of Stratificational Grammar for Contrastive English - French Studies

by

Rose Mary Babitch

Collège Jésus-Marie
Shippagan, N.B.

Attempts at a pedagogical application of the Transformational-Generative theory have for the most part, remained as ineffective as the Structuralist theory criticized by Chomsky, in activating students' ability to think in a foreign language.

The Prague School of Functional Linguistics holds that since sentence patterns constitute an artificial link between habitual use of the mother tongue and the usual devices of the system of the foreign language, students must be aware of the syntactic similarities and differences of both native and target language systems. The foreign language must be taught against the background of the mother tongue. The syntactic sentence patterns which are learnt as skills, must be supplemented with communicative exercises which are to serve as guided practice in activating thinking in a foreign language. Stratificational theory offers a means of programming pedagogical material which permits the construction of structural and communicative exercises.

One of the important functions of structural exercises is the teaching of grammar. The aural-oral approach of both the American Descriptivist and Functional Structuralist Prague Schools permits the student to learn the use of the correct form on the basis of analogy. An example from French gives a good illustration of this:

since - le cinéma can be rendered as au cinéma
then - le restaurant can also be rendered as au restaurant

Following this, and on the basis of sentence patterns, the student can himself derive the rule *a* ± *le* = *au*. Structural exercises also aid the acquisition of automatic use of the language.

There is however a basic difference between the American and Prague Schools.

The American School first of all compares form, position and distribution of language elements both in the system of the native language character of American pattern drills.

The Prague School on the other hand, takes into consideration both form and content. A sentence is not defined merely as a syntactic unit, but rather as a communicative system unit in which an elementary expression of attitude to some reality is carried out by linguistic means.

The formal orientation of American descriptivism gives rise to the following type of exercise in the teaching of the interrogative.

He is a doctor.	Is he a doctor?
She is a typist.	Is she a typist?
He is a mechanic.	Is he a mechanic?

The functional principle of the Prague School states that students must learn the grammatical system on the basis of:

1. its encoding function that is, going from concept to articulation
2. that they must learn it in the way it actually functions in communication.

This linguistic orientation permits the construction of the following type of exercise in contrast to the previous one.

He works at a hospital.	Is he a doctor?
She works in an office.	Is she a typist?
He works in a garage.	Is he a mechanic?

The Prague School does not reject the use of transforms as a teaching strategy. However, a pedagogical application of the Transformational-Generative theory lays stress on the relationship of various sentence elements rather than phrase patterns. Instead of teaching by analogy, it is felt that analysis effectively leads the student to perceiving the language system. What is more significant, is that Transformational-Generative grammars fail to deal adequately with meaning. To remedy this deficiency, transformational exercises proposed by the Prague School are characterized by a natural communicative connection as well as grammatical content. For example:

I like to dance is transformed into I like to dance but I don't like to waltz, rather than...
I like to dance TRANSFORM I don't like to dance.

Communication exercises include a gradation of open-closed questions. They follow structural exercises and must be alternated with them. Their purpose is to lead a student to think of what he wants to say, and not of how he must say it. From the standpoint of communication, the semantic level must be considered primary since language fulfils its function as an instrument of communication between people through its semantic aspects.

The difficulty in teaching French and English verbs arises from the blending of formal and semantic criteria. It is therefore necessary to start with a clarification of terminology.

1. Tense is the grammatical category used to identity forms.
2. Time is the semantic component used to designate the objective reality of past - present - and future tenses.
3. Aspect is the concept from the speaker's point of view. On one hand it is realized by a verbal form, for example in French, the *passé composé* and *l'imparfait* refer to the fundamental opposition *accompli, non-accompli*. Traditionally it is maintained that this part of aspect belongs to grammar. On the other hand, aspect is

realized as *aktionsart* or *manner of action*, this part traditionally belongs to lexology or word formation.

The fact that *tense* is distinct from *time*, and that aspect *form* is distinct from aspect *manner of action*, shows that we are dealing with a dichotomy between semantics and form, that is, *idéation notionelle*, and *idéation de structure*, terms used by Guillaume.

In the article "The Chronological System of the English Verb" William Diver offers a syntactic interpretation of the *actual use* of verbs in the English language.

All forms of the verb are combinations of tense and meaning. Meaning is embedded in a series of forms. Form is the signal which evokes meaning. Two or more meanings may appear in a single form. Diver's description is founded on a code. Letters refer to meanings, numbers to forms. The sequence of letters in the meaning is not significant, the arrangement is arbitrary to reveal the structure with maximum clarity. Meaning "A" (past), stands at the intersection of the axes of the system. The line beginning with "A" (past) and extended to "E" (future) indicates the major horizontal axis. "A" is also the common element of the minor vertical axis which is characterized by the marks below.

R - indefinite

S - definite

T - repeated

K - extended

C - distant

Meaning "A" (past) along the horizontal axis is incompatible with meanings present and future with reference to the same event. Along the vertical axis "A" can be added to marks and combinations of marks, example - AT has the meaning of past repeated.

The arrangement of numbers is significant. The final number represents a suffix, the preceding numbers represent auxiliaries in the order of their occurrence. Thus signal 31 represents the form have - ed on one hand, and on the other, the meaning AR - past indefinite rendered by, "he has walked".

The code proposed by Diver is operational in determining whether a sentence is acceptable or unacceptable. Before applying the code one must know that a relation of exclusion exists between marks R and S, in other words, that the two are incompatible. The following is an application of Diver's code.

(A)	given - marks	R - indefinite
		S - definite

(B) Test of acceptability of the following sentences:

1. The Yankees have won the World Series in 1971.

2. The Yankees have won the World Series.

Test 1 - S_1

Verb - have won - Indefinite - R

Ex: V. Ind. - in 1971 - Definite - S

$R \neq S$

$\therefore$ S_1 is not acceptable.

Test 11 - S_2

Verb - have won - Indefinite - R

Ex: V. Ind. - $\emptyset$

$\therefore$ S_2 is acceptable.

The value of Diver's description lies in showing that all forms used in connection with - past, present, and future have other meanings which correspond with their temporal distribution. Diver's description succeeds in codifying semantic elements, however the relation between meaning and form is limited to the external function of the verb based on relations of exclusion and inclusion.

Paul Imbs in the text, "L'Emploi des Temps Verbaux en Français Moderne", is also interested in analysing the verb according to its actual use in language.

Using modern authors, he first makes a survey of French verb forms and their uses, "les valeurs temporelles". Some of the uses are in a 1 to 1 correspondence with Diver's Temporal Meanings. The second part of the work consists in systematically grouping verbal forms according to meaning. This is done within the framework of Guillaume's theory. What is relevant to my article is that both Imbs and Diver make a distinction between Time and Tense.

According to Imbs, time is a *value*, tense is the expression of a *temporal form*. Aspect is also conceived as both *value* and *form*. As *value*, it corresponds to Diver's Temporal Meaning. As *form* it represents the fundamental opposition between *accompli* and *non-accompli*, that is a simple form as opposed to a compound verbal form for example *je viens* as opposed to *je suis venue*.

The structure of a verb according to Imbs is twofold, one part includes *le sémantème*, example *le radical chant*, the other part is made up of *la variable morphologique*, example: the ending *ais* or the auxiliary *suis*. The variable morphological features belong to grammar. The functional meaning of a verb exists not only in relation to *le sémantème* and *la variable morphologique*, but it exists also in relation to aspect. The ultimate realization of verbal meaning is at the level of the sentence where it is defined not only by the tense form, but also by extra verbal indicators.

Imbs' description is detailed, pertinent and interesting, but it is not as functional as Diver's. That which makes Diver's analysis functional is the economic codification of semantic components. Implicit in both Imbs' and Diver's analyses of the verb is that there are two systems at work, one semantic the other formal. In their descriptions of the combination of the two systems, each has grasped, one for English the other for French, the workings of surface relationships between meaning and grammar. Both Imbs and Diver mention equivalent semantic components which permit the outlining of an internal verbal structure.

I postulate that semantics and syntax are two separate systems, and that a theory of levels can adequately describe the interrelation between meaning and grammar. I propose that this can be done within the framework of Stratificational theory.

The information supplied by Imbs and Diver permits us to graphically represent the verbal structure of both French and English at the level of the sememic stratum.

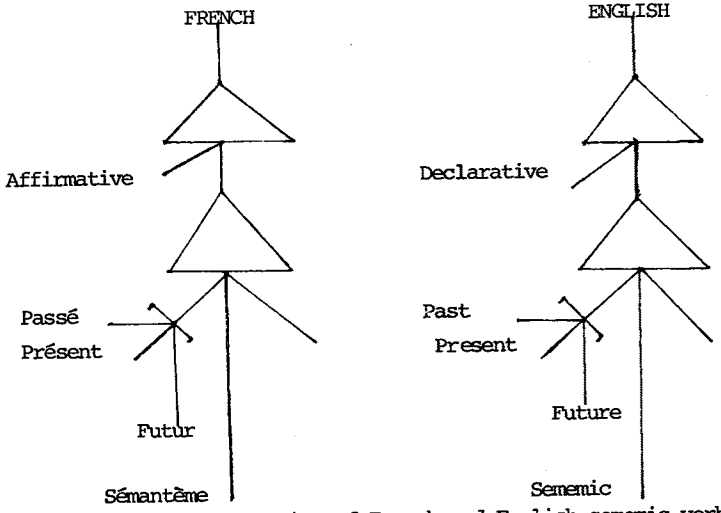
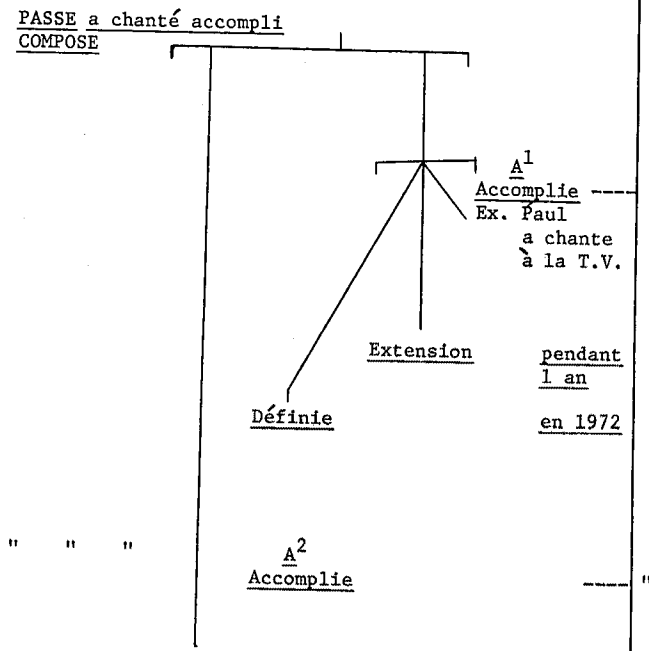
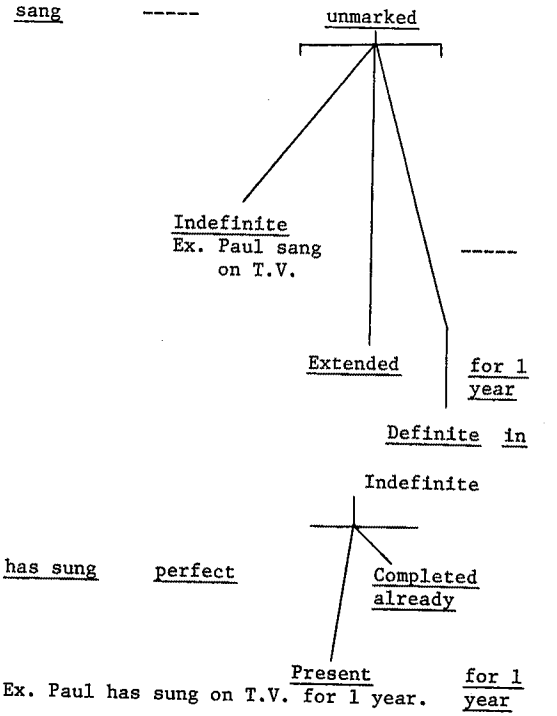


Figure 1: Representation of French and English sememic verbal structure[1].



Examples:

1. V.T. Extension: Paul a chanté à la T.V. pendant 1 an
2. V.T. Définie: Paul a chanté à la T.V. en 1972
3. V.T. Accomplie: Paul a chanté à la T.V.
4. V.T. Accomplie: Paul a chanté à la T.V.
5. V.T. Présente: Paul chante à la T.V.



1. T.M. Extended: Paul sang on T.V. for 1 year
2. T.M. Definite: Paul sang on T.V. in 1972
3. T.M. Indefinite: Paul sang on T.V.
4. T.M. Completed: Paul has already sung on T.V.
5. T.M. Present: Paul has sung on T.V. for 1 year

CONTRASTIVE TABLE [2] -- containing corresponding semantic equivalents of French and English Past Tense verbs obtained by the matching of Imbs' Valeurs Temporelles with Diver's Temporal meanings

FRENCH					ENGLISH				
TEMPS PASSE					PAST TENSE				
TPS	FORME	ASPECT	V.T. (VALEUR TEMPORELLE)	PERIPHRASE	TNS	FORM	ASPECT	T.M. (TEM. MEANING)	E.V. IND (EXTRA VERBAL INDICATOR)
IMPFT.	chantait	non-accompli	éloignée	autrefois	PAST	USED to sing	-----	distant	-----
	Ex. Autrefois, Paul chantait.					Ex. Paul used to sing.			
IMPFT.	chantait	non-accompli	spécifique	-----	PAST	was	singing progressive	definite	-----
	Ex. Que faisait Paul à la T.V.? Il chantait.					Ex. What was Paul doing on T.V.? He was singing.			
IMPFT.	chantait	non-accompli	répétée	chaque fois	PAST	Would sing	-----	repeated	-----
	Ex. Que faisait Paul pendant chaque conférence? Chaque fois, il chantait.					Ex. What would Paul do during the lecture? He would sing.			
IMPFT.	continuait à chanter	non-accompli	continue		PAST	kept on singing	progressive	extended	-----
	Ex. Paul s'arrêtait-il à la cloche? Non, il continuait à chanter.					Ex. Did Paul stop at the bell? No, he kept on singing.			

NOTE: continuait à chanter - internal verb structure = two sémantèmes.

Using the contrastive table, the relation between the sememic and lexemic strata can be represented as follows:

Fr.	TEMPS	SEMANTEME	VALEUR TEMPORELLE
Sememic	PASSE	CHANT	ELOIGNEE
Lexemic	PAUL	CHANT - AIT	AUTREFOIS

Eng.	TIME	SEMEME	TEMPORAL MEANING
Sememic	PAST	SING	DISTANT
Lexemic	PAUL	USED TO SING	

Figure 2: Represents Lamb's Relational Approach.

In the teaching of either French or English verbs, both the contents of the contrastive table and the relations between the sememic and lexemic strata are of use in the preparation of teaching units. The tactics used in teaching to develop the awareness of the target language should be to proceed from what is common to the native and target languages to what is less similar, and further to what is contrarily opposed. For the teaching of the English past tense, the proposed outline of teaching units is as follows:

Unit 1: Verb - read

	TENSE	FORM	$MEANING_1$	$MEANING_2$
Fr. :	Passé composé	a lu	définie	accompli
Eng.:	Past	read	definite	indefinite

Construction of structural exercises.

Unit 11: Verb - work

	TENSE	FORM	MEANING
Fr.:	Passé composé	a travaillé	extension
Eng.:	Past	worked	extended

Structural exercises are supplemented with communicative exercises constructed with meanings - definite, indefinite, extended.

Unit 111: Verb - wait

	TENSE	FORM	MEANING
Fr.:	Imparfait	attendait	spécifique
Eng.:	Past	was waiting for	definite

Structural exercises are followed by communicative exercises which include use of meanings learned thus far.

Unit lV: - would include English Past meaning *distant* with the French equivalent as seen in the table.
Exercises follow.

Unit V : - would include English Past meaning *repeated*.

Unit Vl: - would include English Perfect with meanings *completed and present*.
Here the students encounter a feature opposed to their native tongue. Example:

Il habite Paris depuis deux ans.

He has lived in Paris for two years.

	TENSE	MEANING
Fr. :	Present	Present
Eng.:	Perfect	Present

Unit Vll: - would include the English Past *kept on ± ing*, meaning *extended*. The French internal verbal structure pertaining to this form and meaning is characterized by an additional *sémantême*.

Conclusion:

A second language cannot be taught without reference to meaning. In the teaching of French and English verbs, Stratificational notations make possible the structuring of semantic components and a description of their direct relation to grammar. This in turn permits a systemic organization of pedagogical material for the teaching of verbs within the framework of the principles of the Prague School which holds with Stratificational theory that in speech, an act of communication is rendered by the use of a specific linguistic code which permits encoding and decoding on the part of the users.

It is my belief that the structural and communicative exercises constructed at the beginner's level are basic to both speech and writing for they stimulate semological awareness of the target language. It is only after the semological foundation has been laid, can written verb exercises be introduced.

FOOTNOTES

[1] Both traces represent standard international French and English, thus typologically they are very similar. As a consequence, they are quasi-identical.

[2] It is to be noted that the table is open to additions of temporal meanings not included in Diver for example - meaning - Recent Past.

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ORTHOGRAPHIES AND SCRIPTS: A NECESSARY DISTINCTION

Earl M. Herrick
Texas A&I University

A paper by the present author that is now in press (Herrick *in press a*) describes a stratificational outline for analyzing and describing written languages, especially those which have graphonomies of an alphabetical kind. Figure 1 shows, with slight modifications, the general outline that is proposed in that paper for the graphonomy of a language. Like other stratificational descriptions, it is a network organized into sign patterns, alternation patterns, and tactic patterns. Between its patterns, it has various kinds of -emes, -emic signs, and -ons, which represent more-or-less abstract facts about the written marks of the language. Figure 2 illustrates some of these facts, by means of a typical mark from written English and the linguistic facts which describe that mark on four levels of language.

In English and similar written languages, the typical segmental graphemes are the letters. The other segmental graphemes include numerals, punctuation marks, and such marks as the ampersand. A language may also have diacritic graphemes such as accent marks, and suprasegmental graphemes such as capitalization and italicization.[1] On the graphemic level, the mark shown in Figure 2 is described by ^{G}/H/ occurring alone.

Each grapheme or group of simultaneous graphemes is typically realized by two graphemic signs, one representing the shape of the written mark, the other representing the height of the mark with respect to its neighbors. On the graphemic sign level, the mark shown in Figure 2 is described by ^{GS}/minuscule h/, which describes its shape, and by ^{GS}/ascender present/, which shows that it extends above the x-height (which is the height of the shortest lower-case letters).

Every graphemic sign is realized by the minimum number of graphons that will suffice to distinguish it from all the other graphemic signs. On the graphonic level, the mark shown in Figure 2 is described by four graphons. The first three of them realize ^{GS}/minuscule h/; for two of these graphons, the parts of the mark which they describe must also be stated. The second graphemic sign is trivially realized by a graphon of the same name.

On the hypographemic level, a mark is described by one hypographeme for each place or way in which it might have a significantly different shape. The mark shown in Figure 2 is described on this level by eight hypographemes, the first three of which realize, respectively, the first three graphons. The next three hypographemes do not realize any graphons, but are provided by the hypographotactics. On this level, as on the level above, the parts of the

[1]The present paper will discuss only the letter graphemes and their accompanying diacritic or suprasegmental graphemes. The numerals, punctuation marks, and other graphemes will form similar but separate systems.

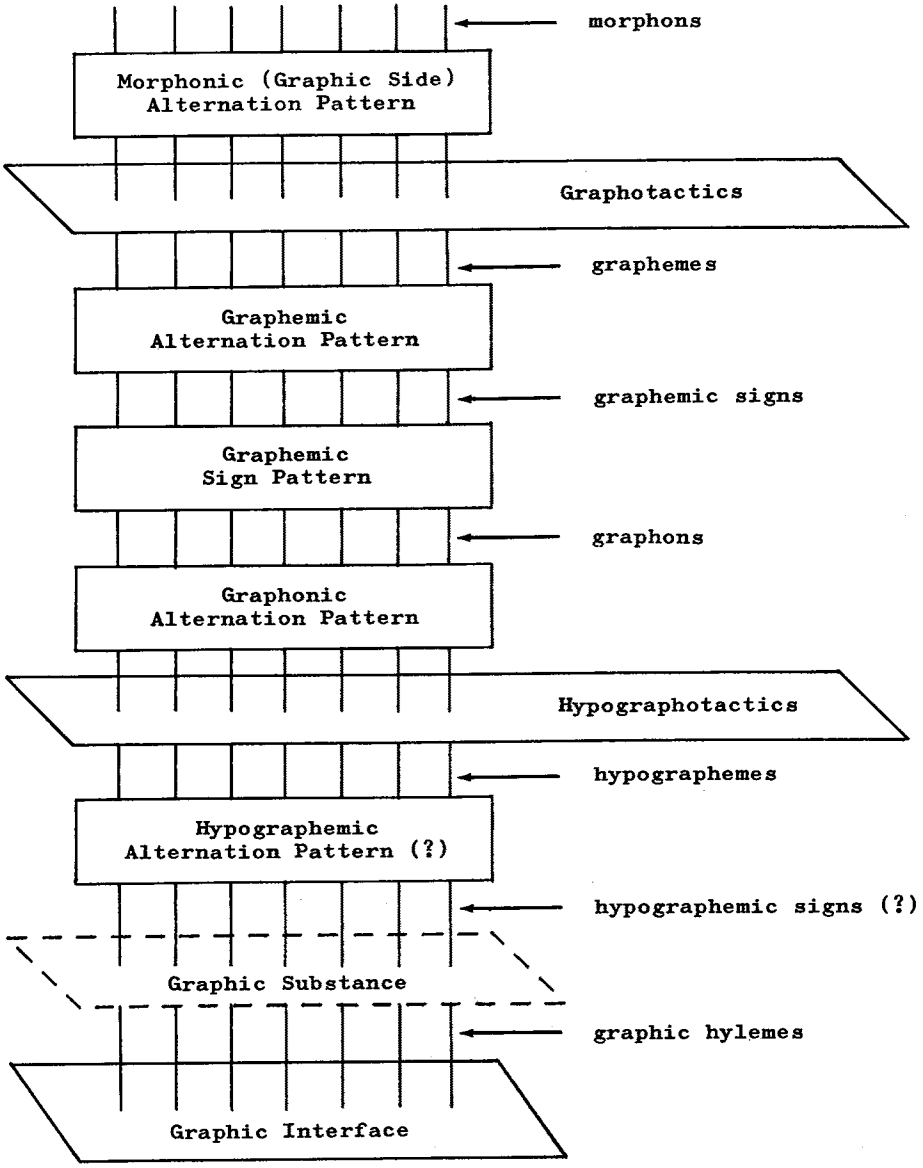
morphons
Morphonic (Graphic Side)
Alternation Pattern
Graphotactics
graphemes
Graphemic
Alternation Pattern
graphemic signs
Graphemic
Sign Pattern
graphons
Graphonic
Alternation Pattern
Hypographotactics
hypographemes
Hypographemic
Alternation Pattern (?)
hypographemic signs (?)
Graphic Substance
graphic hylemes
Graphic Interface

FIGURE 1.

mark which some of these hypographemes describe must be stated. The last two hypographemes refer to the height of the mark and realize ^{GN}/ascender present/.

Figure 2 does not give examples for all the parts of a graphonomy shown in Figure 1. The author has recently found a few cases of free or stylistic variation that should perhaps be handled below the hypographemes; he has therefore tentatively included a hypographemic alternation pattern and hypographemic signs in Figure 1. At the bottom of that diagram, there is a graphic substance, which is the place where the marks of written language actually occur. It is a Hjelmslevian substance (Hjelmslev 1961: 52), outside language but with a structure provided by the language. The graphic hylemes indicated in the diagram represent measurements that can actually be made on the marks in the graphic substance.

h

Graphemes: ^{G}/H/

Graphemic Signs: ^{GS}/minuscule h/
^{GS}/ascender present/

Graphons: ^{GN}/upper side closed/
^{GN}/straight extension/: MAJOR DIAGONAL
^{GN}/rounded corner/: MINOR DIAGONAL
^{GN}/ascender present/

Hypographemes: ^{HG}/opening at bottom/
^{HG}/left curved extension at corner/: UPPER LEFT
^{HG}/rounded corner/: UPPER RIGHT
^{HG}/no loops/
^{HG}/one most-surrounded region of background/
^{HG}/non-concave edge/: UPPER MIDDLE
^{HG}/top edge above x-height/
^{HG}/bottom edge at x-height/

FIGURE 2.

Figure 1 includes, at its top, morphons and a morphonic alternation pattern. This pattern will handle discrepancies such as the morphographemic alternation which occurs in written Spanish between ^{G}/C/ and ^{G}/QU/; this alternation will be handled in this pattern as the realization of a single morphon.[2] The speech/writing disjunction clusters, which occur at the top of the graphonomy, have been omitted in Figure 1 because they can occur above this diagram or inside the upper patterns of it. These clusters are the places in the network where the downward transduction of an utterance diverges toward the phonology or the graphonomy, according as the person producing the utterance chooses to speak it or write it.

If every language were written by a unique system of marks, a separate graphonomy of this kind would have to be described for every written language. For a language such as Korean, which has a writing system that is not used by any other language, that is what must be done. Most written languages, however, do not have such unique writing systems.

Let us consider the usual situation from the point of view of a literate person learning a second written language. When, for example, a person literate in English sets out to learn to read and write Spanish, he does not have to learn an entirely new system of written marks as he would, for example, if he were to set out to learn written Chinese. He does have to learn the spellings of Spanish words, and he has to learn something about the allowable combinations of Spanish graphemes. He also has to learn to identify a few marks, such as the tilde, the dieresis, and the acute accent. He will discover, however, that because he knows written English, his linguistic competence already contains what he needs to know in order to read and write all the other marks of written Spanish. Indeed, a person who knows the right written languages may be able to learn another written language without having to learn any new marks. A person literate in both French and Spanish who sets out to learn written Portuguese will encounter certain familiar marks in unfamiliar combinations, but he will not encounter in that language any marks which he does not already know.

As it is with a literate person learning another written language, so it is with linguists describing such languages. If there were available a complete linguistic analysis of written English, it would be a waste of time to make, de novo, a complete analysis of written Spanish, written French, written Ibo, written Hawaiian, modern written Turkish, or any other written language using the same basic system of marks. Any of these languages would require a separate analysis of its graphemic inventory, its graphotactics, and its morphemic sign pattern, all of which are in the higher parts of its graphonomy. But, for each of these languages, the analysis of the shapes of its marks could and should be borrowed, largely or completely, from the lower part of the available description of written English.

Let us consider some further differences between the higher and lower parts of certain graphonomies. If there were a linguistic analysis of the morphographemics of Spanish, the graphotactics of Hungarian, or the phoneme-grapheme fit of Tibetan; and if a newer, more elegant analysis were made for that part

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[2]For an analysis and description of this and other morphographemic alternations in Spanish, see Herrick *in press b*.

of that language; the newer analysis would affect only the Spanish, Hungarian, or Tibetan language, as the case might be. However, if there were a linguistic analysis of the features that distinguish English GS/minuscule h/ from all other marks of written English, and if a newer, more elegant analysis were made of those distinguishing features, the usefulness of the newer analysis would not be limited to English, but would be equally valid for every other language using the same system of marks.

Furthermore, there are times when the whole upper part of a graphonomy may be realized, lower down in the same graphonomy, by alternative systems of marks shared with different other languages; examples of such realization are found in Serbo-Croatian, German, and the languages of India. Serbo-Croatian has two systems of written marks--Roman and Cyrillic--which are completely interchangeable, so that a text written in marks of one kind can be converted into marks of the other kind by simple, mechanical substitutions. An analysis of written Serbo-Croatian must therefore assume that every word has a single, constant realization on some higher level of the graphonomy, which on the lower levels of the graphonomy is realized by either Roman or Cyrillic marks, according to the writer's choice.

The change which has recently been completed in German from Fraktur marks to Roman marks did not affect the graphemic analysis of German. It affected only the shapes of the marks which realize its graphemes; the graphemic inventory of German, its graphotactics, and all the other higher parts of its graphonomy are the same after the change as they were before.

In any of the languages of India which borrow their learnèd words from Sanskrit, tatsamas from Sanskrit and other words of the language are all written with the marks of whatever system is peculiar to that language. However, whenever one Sanskrit word is borrowed as a tatsama into several of these languages, the sequences of marks used to write that word in each language will be letter-for-letter transliterations of one another. We must therefore assume that each such Sanskrit word has a single, constant realization on some higher graphonomic level, and that differences among the marks used to write it result from differences in the lower parts of the graphonomies of the borrowing languages.

These phenomena should help to demonstrate that, within the graphonomy of every written language, there are two kinds of linguistic phenomena. Each graphonomy will have some upper parts which can belong only to its own language, while it will have some lower parts which may be shared with other written languages. The higher part of a written language, which cannot be shared with any other language, may be called the <u>orthography</u> of the language; the lower part of a written language, which can be shared with other languages, may be called the <u>script</u> which the language uses.

The author would suggest that the orthography and the script of a written language are logically separable, and that linguists can and should analyze them separately. A linguist should be able to analyze a script, and the contrastive features which distinguish its marks from one another, without having to deal at the same time with the linguistic features peculiar to any one language. On the other hand, a linguist should be able to analyze the features of an orthography which are necessarily peculiar to its language without having to describe the exact details of the shapes of its marks. The distinction between an orthography and the script which it uses should be made at the graphemic

level. The orthography of a language should include its graphemic level and all of the graphonomy above it; the script which a language uses should include the graphemic level of the language and all of the graphonomy below it.

A description of an orthography should include the following six things:
(1) A complete inventory of the graphemes of the language, including its letters, its diacritic graphemes if any, its suprasegmental graphemes if any, and all of its other graphemes.
(2) A complete graphotactics, stating how its graphemes must, may, or cannot cooccur, simultaneously or in sequences.
(3) A complete description of the realizational patterns that occur between the graphemes and the speech/writing disjunction clusters of the language, including the morphographemic alternations and all the other discrepancies located in that part of the graphonomy.
(4) The names of its letters.
(5) The alphabetical order of its letters.
(6) The pronunciations of its letters, and perhaps of its other graphemes, specifying the phonemes used to pronounce each of its letters and the letters used to write each of its phonemes.[3]

A description of a script should include a graphemic level, with an inventory of graphemes and a graphotactics, and everything that appears below the graphemes in Figure 1, viz. the graphemic alternation pattern, all the lower parts of the graphemic stratal system, and the entire hypographemic stratal system, including the graphic substance and the projection processes across the graphic interface.

When an utterance is being transduced, the relationship between an orthography and its script at the graphemic level where they adjoin is no problem. The graphemes of the orthography are identified with the corresponding graphemes of the script which it uses, and the transduction proceeds through smoothly. In a linguistic description, the graphemic levels of an orthography and of a script are quite different, although each contains a graphemic inventory and a graphotactics. The graphemic inventory of a typical script is larger and more inclusive than the graphemic inventory of any one orthography that uses it. The inventory of an orthography contains only the graphemes used by its own language; the inventory of a script contains every grapheme that is used by any language which uses the script. On the other hand, the graphotactics of a script is smaller and less powerful than the graphotactics of any orthography that uses it. The graphotactics of an orthography describes completely all the ways that its graphemes can cooccur; the graphotactics of a script merely states in general terms how its graphemes can cooccur simultaneously.

The idea that one linguistic analysis should be used in describing parts of several languages may seem to violate the old, solid, linguistic principle that every language must be described on its own terms. Unfortunately, that is the

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[3]The discussion in this paper has assumed that each letter has a name, a place in an alphabetical order, a function in the graphotactics, a pronunciation, and (indirectly) a shape. The reader should be warned that the letters (or units, whatever they may be called) which a language uses for these several purposes are not necessarily the same. The author is writing another paper on this topic.

way graphonomies work. The borrowing of written marks from one language to write down the sounds of another language is a long-standing practice, which goes back through classical times to the days of cuneiform. And since the invention of moveable type, fonts of identical types producing identical marks have regularly been used to produce written texts in many different languages. To the extent that this has been done and can be done, scripts can be and should be described separately from orthographies.

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MUTATIONS AND RELATIONS

Sydney M. Lamb

Yale University

and

Center for Advanced Study in the Behavioral Sciences

> What is ordinarily called phonetic "change" or "transformation" of one sound into another is, from an objective point of view, only coexistence, or alternation.
>
> Baudouin de Courtenay (1895)

Most linguists would probably agree that the structure of any language consists at least in large part of relations; but there is considerable disagreement with respect to how such relations should be described. A prevailing tendency throughout the Boas-Sapir-Bloomfield-Harris-Chomsky tradition has been to treat certain types of relations mutationally; that is, to account for linguistic entities by deriving them from other entities through the operation of processes. Such terms as phonological process, morphophonemic change, rewriting, deletion, and transformation illustrate mutational thinking. The practice has become increasingly widespread during the recent stages of this tradition, and it is widely supposed that certain linguistic phenomena simply cannot be described without resorting to mutational devices, e.g. transformations.

As previous critiques of mutational description, by myself and others, have failed to dissuade many practicioners and have led to various misconceptions, a more detailed treatment is attempted in this paper, which analyzes the nature and ramifications of mutational description and which shows that converting from a mutational description of a language to a

directly relational one has consequences extending far beyond the treatment of individual linguistic phenomena.

First, let us consider the question, is there an alternative to mutational description?

Those who are acquainted with the literature of cognitive linguistics will have no doubt about what the answer to that question is. For the sake of those who aren't, perhaps a little repetition will be permitted---but instead of just repeating, I'll try to state the case somewhat differently, I hope more clearly than before.

Let's first take a look at a non-linguistic example. Consider the definition of the circle. Many people have been brought up with a mutational definition, one which embodies a process: "A circle is the line made by a point moving at a fixed distance from another point." To many of those who have learned only this definition, it may seem that there is no way to do it without the process. The circle is then conceived of as a dynamic, moving, thing---if the definition is taken seriously; and if it isn't, then they are in the position of believing something different from what the words say. But there is a way of defining the circle without process, so that it is a static entity, a motionless curved line: "A circle is the locus of points equidistant from a given point."

Turning now to language, let us next consider morphophonemics. The alternation f ~ v in <u>knife</u>:<u>knives</u> provides an example for discussion. The mutationist would say that f changes to v , or that f is rewritten as v , or the like. (Strictly speaking it is to be done in terms of phonological components or distinctive features, with s ~ z (<u>houses</u>) and θ ~ ð (<u>baths</u>) to be covered by the same rule, but that consideration isn't directly relevant to the present discussion.)

By now it is finally well known that, for morphophonemics, alternatives to mutational description do indeed exist. The proposals of alternatives go back at least as far as Harris' "Methods in Structural Linguistics" (1951) in this country and at least as far as Baudouin de Courtenay in Europe. But the rather dramatic consequences of the difference (see below) still remain matters of widespread ignorance.

Any case of morphophonemic alternation involves a relation, and, like the circle, it can be completely specified without the introduction of any motion or change into the specification. The relation can be described directly---as a relation: we have f and v in alternation as alternate realizations of a higher-level (or 'underlying') item. (Again, this example would actually be done in terms of phonological components, as alternation between voicing and zero.) At the higher phonological level, <u>knife</u> has only one form. This purely relational treatment corresponds to that which can be used for subphonemic ('allophonic') alternation and morphological alternation like that of gud and bet (of <u>better</u>) as alternate realizations of a higher-level <u>good</u>.

An important lesson to be learned from the experience of linguistics with morphophonemic alternation is that we must be careful to distinguish the phenomenon from the means of describing it. There have been those who believed that the phenomenon of morphophonemic alternation was a process, since a process approach was used to describe it. Such linguists decided that if process description is to be avoided, then morphophonemic alternation as such had to be ignored; and the only alternative was the grossly uneconomical description involving morphemes and allomorphs.

Is there also a non- mutational alternative to transformations? As I have shown on various occasions, beginning with a paper presented to the Linguistic Society of America at its annual meeting in 1960, there is indeed. The same line of reasoning that leads us to recognize a phonemic stratum 'behind' the phenomena of phonetics, a morphophonemic stratum 'behind' the alternations among phonemes, can be applied to the phenomena commonly described by means of transformations.

Here we must keep in mind the lesson I referred to in connection with morphophonemics. Many have supposed that transformations are the phenomena, rather than the means of describing the phenomena. Supporters of transformational description have often supposed that those who reject transformations are therefore failing to recognize the phenomena. And on the other hand some of those who have found transformations unrealistic have, since they were involved in the same confusion, decided against recognizing the phenomena.

But the phenomena described by means of transformations involve alternation. They are therefore open to the same kind of treatment that is available for describing alternation at other levels of linguistic structure.

Consider active and passive clauses of English; for example,

notation influences thinking and

thinking is influenced by notation .

Now in the first place they are not identical in meaning: the former is a statement about notation while the latter is a statement about thinking. But they are very similar in meaning, and that fact needs to be accounted for. The relation of <u>notation</u> to <u>influence</u> is the same in both---it can be labelled as the <u>agent</u> relation; and that of <u>thinking</u> to <u>influence</u> is likewise the same in both---this can be called the <u>patient</u> relation. Using a purely relational approach to describing the phenomenon, we can say that these active and passive clauses exhibit alternations giving evidence of a higher level of structure--- the level of the units having these realizations. Thus the higher level evidently has an <u>agent</u> element, which is realized as <u>by</u> in the passive clause but not in the active.

Consider also such phrases as <u>the shooting of the hunters</u> . The transformational approach to accounting for the ambiguity is to derive the phrase from two different sources by transformations (e.g. Chomsky

1957). The purely relational description is like that used at lower levels. Just as -d of English is ambiguously the realization of either past tense or the past participle marker, so the of in the shooting of the hunters is ambiguously the realization of either the agent element or the patient of the deep grammar.

At this point a small digression is necessary, concerning the levels of deep structure and surface structure, or, as Hockett (1958) called them, deep grammar and surface grammar. From what I have just said it appears that the alternative to transformational description is to distinguish these two levels; yet transformational grammar does speak of such a distinction, while at the same time using transformations as well. It thus might seem that we need both the stratification and the transformations. The transformations would be the means of converting deep grammar representations into surface grammar representations. A little glimpse into history may clarify the situation. In Chomsky's original formulation (1957) these levels were not distinguished. That system may be called a purely mutational one. The original version of stratificational grammar presented to the LSA in 1960 proposed that, to deal with the phenomena being described by transformations, a higher stratum of linguistic structure be recognized. Similar proposals were beginning to be heard within the transformational camp at around that time. Then in the early sixties Chomsky promulgated the 'deep structure' concept (possibly borrowing the term from Hockett). But he retained the transformations. Now if one is really recognizing different levels of structure, then the transformation, an inherently mutational rule, is not the appropriate kind of rule at all. Instead, we need a rule suited to stating realizational relationships. And by the same token, if one does persist in using transformations, then one is not really making a clear distinction between the two levels. Thus Chomsky's deep structure is seen, upon inspection, not to be very deep after all. It is made up largely of elements and categories of surface structure. In short, the revision which introduced the concept of deep structure into transformational grammar was a step in the direction of recognizing a stratal distinction, but only a step, since the transformational rule was retained. The generative semanticists have gone a little farther with their so-called semantic level, which is a little deeper; but they still relate it to surface structure with mutational rules, and, in keeping with the nature of those rules, it still retains many features which really belong only to surface grammar.

Continuing with the question of whether an alternative to mutational description exists, let us briefly consider deletion, a process used by mutationists at various levels of structure. The non-mutational approach has two ways of treating the phenomenon: some cases involve zero realization, while others are simply instances of omission. To those whose thinking runs along mutational lines it may seem that there is little or no difference between deletion and omission; but in fact the distinction is of the utmost importance. Deletion is the removal of something that was there before the deletion took place. Omission, by contrast, involves no such operation. The omitted item is never put there in the first place. Deletion implies a prior operation, in which the item is put 'there', and the deletion itself is the subsequent process of removing it.

Zero realization is just alternation in which one of the alternate realizations is zero, or nothing. The plural element has a zero realization when occurring with sheep or deer . On the higher level there is something; when the context (e.g. sheep) calls for a zero realization, it does not get deleted---on its own level it remains present; the zero realization is on the next lower level.

Let us now pause for a moment to see where we are and where we are going. I have shown so far that the various kinds of phenomena which have customarily been described in the mutational framework can also be described in purely relational terms. It thus appears that we have two alternative modes of description, and it is natural next to ask such questions as whether one is preferable to the other in any way, whether they are really different in some significant structural way, whether one is more realistic than the other. Let us proceed next to this last question. Let's put it this way: Is mutational description realistic?

The answer, which I shall now attempt to justify, is that the mutational approach is at variance with reality, except when applied to situations in which change actually occurs.

Consider first morphophonemic alternation. Who changes f to v ? The speaker? When? Every time he utters the form knives ? Surely not. As Bloomfield writes in his book "Language" (1933),

> "... it goes without saying ... that the speaker who says knives , does not 'first' replace [f] by [v] and 'then' add [-z], but merely utters a form (knives) which in certain features resembles and in certain features differs from a certain other form (namely, knife)."

As an aid in appreciating the unrealness of mutational thinking, let us consider a non-linguistic example. Identical twins look very much alike. Now is it the case that one of the pair of twins is derived from the other by the operation of some process?

Similarity, even very close relationship, does not have to mean that one is derived from the other. There are other conditions which can give rise to similarity. Now let the mutationist look at the various linguistic situations in a new light; and he will see that there too what we have is similarity, shared properties---if you will, shared connections to a common higher-level (or 'underlying') element---rather than mutation.

Let us look at deletion in connection with the reality question. Who deletes the deleted item? The speaker? When? When someone uses deer or sheep in the plural, does he first put on a plural ending and then delete it?

Again perhaps a non-linguistic example will make the absurdity of mutational thinking stand out more clearly. Consider the case of the dinner in which, for one reason or another, there is no dessert. Does

the cook bake a pie and then throw it out? Of course not; the dessert is simply omitted. Such non-linguistic examples are so clear precisely because they are non-linguistic. The person trained in mutational linguistics has become habituated to mutational description of linguistic material and has perhaps at times reconciled his concern for reality with what has been taught to him by unconsciously providing for himself some kind of translation into a non-mutational conception behind the mutational words. He thus becomes progressively less aware of the inappropriateness of the mutational description. And so I resort to non-linguistic examples to jar him out of this complacent condition, in which he has accepted words as meaning something other than what they say.

Now on the other hand, there are real mutations. Its not that mutation is bad in itself, or mistaken. Rather, the mistake is the failure to distinguish between static relations on the one hand and real processes on the other. The latter do indeed occur. Linguistic encoding and decoding, the processes of language use; linguistic change. Ironically, the mutationists, who might be thought to be quite interested in processes, are the least able to deal with these real processes, simply because they have their linguistic information hopelessly entangled with fictitious process.

There is, in reality, such a thing as deletion, as a real process. It occurs, for example, when a writer writes down a passage of prose on a piece of paper, and then, later, deletes a word or a phrase.

Again let us pause to see where we are. It appears, first, that an alternative to mutational description exists---one may account for the same phenomena realizationally. Moreover, the realizational approach is not just a co-equal alternative, since the mutational approach has an important defect---it fails to correspond with any known or supposed reality. And indeed it has often been admitted by mutationists that the processes of their descriptions have nothing to do with the real processes which occur when a person speaks.

Is there then any excuse for mutational thinking? Any reason to justify the continued use of mutation rules by our colleagues? There are probably three main reasons why mutationists are still active in our midst. First is that which I already mentioned---some have apparently provided internal translations into non-mutational concepts and have been willing to use the words and notations without regard for what they are really saying. Second is the belief that there is no other way to describe the phenomena. But of course now we know better: These phenomena involve relations, and those relations can be described directly, in their own terms. The third reason is that mutational thinking is what they were taught when they were learning linguistics, and it seems to be a general rule in academic life that one doesn't change his fundamental beliefs or practices except under extreme duress. In short, the third reason is simply inertia.

Now the mutationist, at this point, not wanting to be considered guilty of unmotivated conservatism, might defend himself by arguing that, despite its failure to be realistic, mutational description is convenient, familiar, and easy to use; and as long as we keep in mind that it is unrealistic it will do us no harm to use it, just as a convenient descriptive fiction.

But this position is untenable, for two reasons:

(1) Mutational description influences the thinking anyway, even if the linguist is making a conscious effort to avoid such influence. Thus we find Chomsky (e.g. 1965:47-59) believing that something very much like a system of transformational rules is actually present in the mind. And he is surely to be regarded as one of the more sophisticated thinkers of that school; and he has repeatedly admitted that his rules are not supposed to represent what the speaker actually does when speaking. What we see here is the principle of the Whorf hypothesis at work, but in an extended version. According to the basic Whorf hypothesis, a person's thinking is in general influenced by the structure of his language. The extended version I propose, or perhaps it could be called a corollary, states that a scientist's thinking about his subject matter is influenced by the notation he uses. He cannot avoid it, even if he tries. It is therefore important for the scientist to be very careful about choosing a system of notation; he should eschew any system which has features---such as arrows and hidden assumptions---that are not appropriate to what he is working on. (The major hidden assumption of mutational notation is that linguistic representations are derived from other linguistic representations.)

(2) A mutational formulation is structurally quite different from a purely relational one. It is not the case, as has been claimed, that they are merely notational variants of each other. And because they are structurally different, the choice of the one or the other has consequences extending far beyond the description of individual cases of alternation.

I would like now to give an analysis of the structural differences between the two formulations, and we can then consider some of the more important consequences of the difference.

Let us consider a general case, in which we have two alternants, x and y. And let us say that the alternant y occurs in some restricted (or otherwise easily statable) environment. Under these circumstances the mutational analyst says that in that environment, x is replaced by (or rewritten as) y .

I now list five important structural differences between the two modes of description.

1. In the mutational account, x is the item which is there before the mutation. But the mutation operates only under certain conditions, so we should also consider the item which is there if it doesn't operate, after the time when it would if it did. This is of

course also x , the same item as that existing before the mutation. They are one and the same item. (It is in consequence of this feature that the so-called deep structures of transformational grammar are made up largely of items of surface structure.) If we now consider the realizational account, we see three distinct elements; in addition to x and y , the alternants, there is the element on a higher level, which is distinct from x . Let us call it X . That is, the x and the X of the realizational account correspond to one and the same element of the mutational account. If y is zero we can speak of zero realization of X , and in this situation the mutationist would say that x is deleted. The attractiveness of equating X with x , for those who find it attractive, probably arises from the influence of morphophonemic alternations in which a single component is in alternation with zero, as in the example of f ~ v . In such cases (which are quite common) the higher-level bundle of components does indeed correspond to one of the lower-level bundles. But the actual alternation involves components rather than segments; that is, the X , x , and y in such cases stand for components, and the correspondence of the higher-level bundle to one of the lower-level bundles is clearly shown in the realizational dexcription.

2. If the process applies, in mutational description, then y is present and x is no longer there. (This property has important consequences for the question of rule-ordering---it necessitates ordering in many cases which would not require ordering of realizational rules.) In the realizational account, x , y , and X are all there at all times. When X is realized as y , it (X) is still there; y is on a different level. (Thus X is still available to serve in conditioning environments for other rules.)

3. Time is involved in the mutational account, but not in the relational account. In the mutational account, x must be present before y can be present; the latter comes in as replacement for the former. (One might wonder if the realizational account is more complicated than the other since it uses more items---but on the other hand the mutational account requires several time periods, while the realizational account has only one. It is thus clear that no simple-minded measure of economy will suffice to compare the two.)

4. In the mutational account, an operation is built in. The linguistic information does not exist apart from the mutations. In the relational account, by contrast, the information is just static relational information. Now processes---real processes---can and do operate with this information---in particular the processes of encoding and decoding. But they are separate from the linguistic information itself.

5. The fifth and last point of contrast in this list is of particular importance. In order to make the comparison as close as possible, let us consider the relational treatment in conjunction with the encoding process that can be used with it. Then we will have a process operating in both treatments. And of course the two accounts also agree in that a conditioning environment is stated for y , while

x occurs elsewhere. (In other words, the alternant y occurs in the restricted environment; it is under these circumstances that the mutational analyst chooses x as the element to start with.) In the mutational account, then, you first put in x ; later, the rule for the alternation comes up in the sequence of ordered rules, and you check to see whether the conditions are present for the occurrence of y ; if so, x is replaced by y . Now, in encoding with a realizational account, you check these conditions before 'putting in' x ---and if they are present, x doesn't get 'put in' at all. We thus see that X is not really a linguistic item at all, in the usual sense. Rather, it is a state---a position in a network of relations, if you will---the decision point, at which either x or y will be selected depending upon the conditions present. (And it follows that the level on which X exists is distinct from that of x and y .)

It is important that this difference be fully appreciated. I shall therefore indulge in a couple of additional non-linguistic examples. For the usual dinner, in our culture, we have potatoes as the unmarked starch food; but under certain conditions we have rice instead. Now, according to the mutational account, in preparing dinner one cooks potatoes in any case. One then decides whether to have potatoes or rice. If the former, well and good, they are already prepared. If the latter, the potatoes are thrown into the garbage can and rice is prepared. In the realizational mode, the cook decides which it will be before cooking any potatoes.

Or, suppose there is an import-export firm, whose home office is in New York. And from time to time, ships come from the Orient to the West Coast loaded with goods. And suppose that a Mr. Chang flies out from New York to the West Coast to supervise the unloading of the goods from the ship, and their subsequent delivery to a warehouse. Now further suppose that the ships usually arrive at Los Angeles, but that under certain conditions they go instead to San Francisco. Now according to the mutational framework, when word comes in that a ship is arriving from the Orient, Mr. Chang goes out to Kennedy Airport and gets a plane to Los Angeles. Upon arriving in Los Angeles, he telephones the home office in New York to find out whether the ship is coming in to Los Angeles or to San Francisco. If the latter, he gets on another plane and flies to San Francisco. If he operates according to a realizational format, on the other hand, Mr. Chang will find out where the ship is arriving before he leaves New York.

Now let us consider some of the consequences of these structural differences. We can put it this way: Suppose we start with a transformational grammar of a language; let us make this one change---from mutational description to realizational; and then let us see what consequences this change has for the overall form of the description.

1. First, we will find that the structure becomes stratified. In consequence of the recognition of a higher-level entity (position in a network of relations---or decision point, if viewed in the context of the

encoding process) as distinct from the alternants, we have stratification of the linguistic structure. The stratum of X is distinct (though clearly not isolated) from that of x and y. Therefore, the elements of deep grammar, for example, are different from those of surface grammar.

2. Second, we find that the stratum of 'surface structure' has its own syntax or tactics (or 'base component'). If we set out to describe 'transformational' phenomena realizationally---say with realization rules---we soon find these rules looking very much like tactic rules. For those unfamiliar with the term tactic let me explain that tactic rules are rules which specify the arrangements in which linguistic elements occur. They are not rules which tell you that items are moved from one arrangement to another; rather, they simply specify the order in which the items occur. For example, a tactic rule for a traditional old-fashioned railroad train might specify that first comes the engine, then the coal car, then the baggage car, then the diner, and so forth. By contrast, a rule of the transformational type, as applied in the train situation, might specify first the order in which the various kinds of cars arrive in the railroad yard, then the transformations needed to put them into the order in which they actually appear in the train. But it is a property of transformational rules that the order of the result is never given directly---only indirectly, as the outcome of the application of the various rules.

Now similarly, if we consider surface structures of a language like English, tactic rules would specify that sentences are composed of clauses, that a clause consists of first a subject, then a predicate; that in the subject position may occur any of various types of nominal, such as the noun phrase; that a noun phrase consists of (to oversimplify) a determiner followed by one or more adjectives followed by a noun. And so forth. The tactics of surface structure also specifies that there are such things as prepositional phrases, that these occur in various positions in surface structures, such as after verb phrases, and that they consist of preposition followed by nominal. And so forth. Now these used to be well-known facts---perhaps for hundreds of years in traditional grammar---no doubt because they are so obvious upon even a casual analysis.

Yet just this kind of information is nowhere directly stated in a transformational grammar. Instead we find that surface structures are provided by a series of transformations, which specify how to convert from the so-called deep structure to the surface structure.

With this background I can now describe the second consequence more clearly. Let me put it this way: When I set out, some years ago, to account for 'transformational' phenomena realizationally, I first tried doing so with realization rules---rules of a kind that state that so-and-so of the higher level is realized as thus-and-thus on the lower stratum. (The higher level is of course free from variations and peculiarities in ordering of surface structure items which are non-distinctive at this level.) And I found in this attempt that the 'output' side of the

realization rules kept looking like tactic rules; and then I realized that in fact the surface structure ordering can be accounted for by a single integrated set of rules for surface structure tactics, and that a large amount of duplication of ordering information in realization rules can thus be eliminated.

The pursuit of economy thus leads to the conclusion that we must recognize, as did the grammarians of old, that there is a surface structure tactics in addition to the tactics of deep structure (the so-called base component), as an integral part of the linguistic system. And it is this tactics of surface structure---or lexotactics, for short---rather than either transformations or realization rules, that provides for the correct ordering of surface structure items.

For example, consider such phrases as <u>an easy job to do</u>. Now <u>to do</u> goes with <u>easy</u>; that is, the immediate constituents, from the point of view of deep structure, are <u>job</u> and <u>easy to do</u>. Now how do we account for the surface ordering? Does the speaker produce that order because he has a transformation that puts <u>easy</u> in front of <u>job</u> and <u>to do</u> after? Or isn't it rather simply that the surface structure tactics allows the sequence Adjective Noun Infinitive, but not the sequence Adjective Infinitive Noun ?

Perhaps the difference between the two approaches will be made clearer by another non-linguistic example. Suppose that a couple is having a few friends over for dinner. For dinner they'll have baked ham, corn on the cob, salad; for an appetizer, fruit cup; and for dessert, apple pie. The wife makes the pie first, since it takes the longest. The ham is already cooked when bought, so only needs about half an hour to be warmed up. After preparing it and putting it in the oven, she makes the salad. Then the fruit cup. The corn on the cob she cooks at the last minute so it can be served as fresh as possible. So we have

Pie Ham Salad Fruit Cup Corn .

Now how does she know what order to serve the food in? Is it because she has stored in her mind a transformation

$$\underset{1}{\text{Pie}} + \underset{2}{\text{Ham}} + \underset{3}{\text{Salad}} + \underset{4}{\text{Fruit Cup}} + \underset{5}{\text{Corn}} \longrightarrow 4 + 3 + 2 + 5 + 1 ,$$

Or is it because she knows the tactics of the surface structure? That is,

Dinner / Appetizer Salad Meat Vegetable Dessert .

In case there is any doubt about the answer, let us consider the following possibility: Suppose that just as the wife finishes preparing the dinner, after the guests are already present and having cocktails, perhaps, she gets a phone call---her mother has a sharp pain in her side and must be rushed to the hospital. The wife tells her husband, "I've got to go take Mother to the hospital, but you go ahead and serve the dinner and I'll be back later." Now let us also suppose that

the husband knows nothing about cooking, so he doesn't know the order of cooking (i.e., the deep structure). How can he serve the dinner? He doesn't even know the deep structure, let alone the transformation! Nonsense, of course he can serve the dinner---he knows the tactics of the surface structure. No transformation is necessary.

Now let us look at some more linguistic examples. Consider

Herbert doesn't seem to understand Hjelmslev .

Clearly the surface structure is at variance with the meaning, hence with the deep structure, since at the surface level the seem is negated, whereas in the deep structure the negative element goes with understand . How do we account for the surface structure? Very simply, by the tactics of the surface structure itself---which specifies that the clause begins with the subject, which is followed by the main verb phrase, and this phrase consists of optional negative plus main verb (not to mention other possibilities not relevant to this example). It is the lexotactics which places the negative element before the main verb, seem.

Note also that, unlike the example of the dinner or that of the railroad train (which in this respect do not provide suitable analogies to the linguistic situation), the lexotactics is not providing a rearrangement of things that had been differently arranged. The lexical items are arranged at the surface level, not re-arranged. The deep structure does not have the lexical items in a different arrangement---they exist only at the surface level. The level of deep structure is a different stratum; it has semons (concepts) rather than lexical items; these semons provide (through their connections to lexemes) specifications to the lexotactics which determine choices (e.g., of lexical elements) at lexotactic choice points.

Now, a slightly more elaborate example, one which for mutational grammar involves not only transformations but also transformational cycles. Consider first

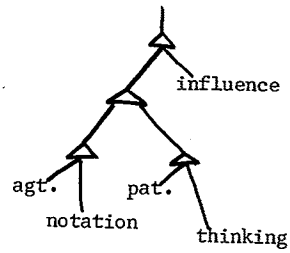
Thinking is influenced by notation ;

and now let us embed this clause in another, so that we get

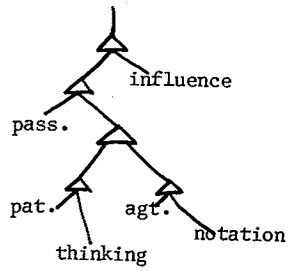
Thinking is said to be influenced by notation .

Again we have an immediate constituent of deep structure realized discontinuously---it isn't thinking that is said; rather what is said is that thinking is influenced by notation. How do we account for the surface order? By the surface structure tactics, which says, in effect, first we get the subject (which must be a nominal), then we get the main verb, together with the passive element, if any. In this case the passive element is there because the patient is the topic---the usual reason for the occurrence of passive. To see more clearly what's going on, we'll have to consider what the semotactic (i.e., deep structure) representation looks like. Let us first take the semotactic representations of

notation influences thinkingand its passive counterpart (with tense and declarative elements not shown):



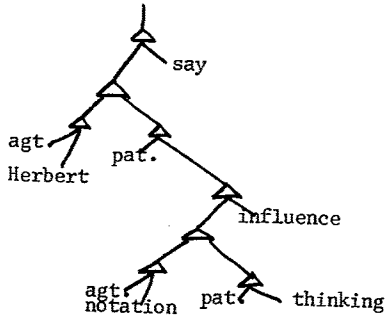
notation influences thinking



thinking is influenced by notation

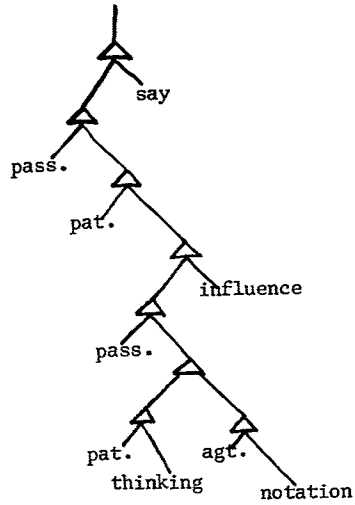
The active and passive are not identical in deep structure---they don't mean the same thing. The active is a statement about notation while the passive is a statement about thinking. In other words, they have different topics. The semotactics specifies that the topic comes first, and the passive element occurs as marker of the construction in which the topic is the patient. (Connections of lines to the bottoms of the triangles in the diagrams indicate whether the constituents are ordered or simultaneous.) Thus the passive element is not meaningful in the usual sense---it is just a marker of a semotactic construction. In all other respects the two deep structures agree: both have thinking as patient and notation as agent of the same process, influence. (In these representations, 'thinking', 'notation', and 'influence' are labels for semons rather that the corresponding lexical items.)

Now a process of discourse, such as ^{s}/say/, can have a thought (e.g., notation influences thinking) as patient:



Herbert says that notation influences thinking

(Again, the tense and declarative elements have been left out of the representation.) Or, the agent of ^{s}/say/ can be omitted and (among other possibilities) the patient of ^{s}/influence/ can be made topic of the embedded thought:



thinking is said to be influenced by notation

Now, given these specifications to determine choices at its choice points, the lexotactics does its thing: First, it needs a subject; the noun thinking is specified by ^{s}/thinking/, the participant which occurs first in the semotactic representation. Then the main verb, say, together with the realization of ^{s}/pass./ if the latter is present (as it is in this example): is said. Then the subordinate verb together with the realization of the passive element if present (as it is in this example), preceded by the empty to, which is required in this environment by the lexotactics: to be influenced. Then the prepositional phrase by notation, in which by is the realization of ^{s}/agt./.

This rather lengthy discussion has dealt with number two in our list of consequences of replacing mutational description by purely relational description. To review the list so far: first, we must recognize the underlying, or higher-level entity of which the alternants are realizations; hence, we recognize stratification in linguistic structure. Second, we see that the stratum of surface grammar has its own tactics (known as lexotactics).

3. Continuing with the list, the third point is that <u>every</u> stratum has its own tactics. This fact remains hidden when one is working with a system that obscures the existence of the strata and which is based on the assumption that the only way to get a linguistic representation is to apply a mutation to some other representation. But when the distortions imposed by the inappropriate notation are removed, the patterns of arrangement on the various strata stand out clearly, and the application of a simplicity principle leads to the recognition of a system of tactics (describable by rules or graphs) for each stratum, which can in itself account for much of the phenomena of alternation associated with that stratum. Rather than going into detail on this one, let us just briefly consider syllable structure. Syllable structure: well-known to generations of linguists---yet accounts of it are strangely absent from transformational grammars. Syllable structure, and the structure of the phonological word, are specified by the tactics of the morphophonemic (or 'morphonic') level, as I have argued elsewhere (Lamb 1966---but with somewhat different terminology). Just as the lexotactics automatically takes care of some of the realizational phenomena between the layers of deep and surface grammar, so the tactics of the morphons (i.e., morphotactics) and that of the phonological components (phonotactics) automatically take care of much of the realizational phenomena of phonology.

4. The fourth consequence concerns the topic which in mutationalist circles is known by the term 'rule-ordering'. It is often argued that rules must be ordered, and one finds that numerous layers of ordering are needed---perhaps as many as 25 for Hebrew phonology, Chomsky (1964) has declared.

Now one might at first glance suppose that, since the translation from a mutational to a relational account involves recognizing a higher level for each rule, we would end up with some 25 strata for Hebrew phonology. We find in fact nothing of the kind. There are several different kinds of situation which lead to a need for ordering of mutation rules, only one of which requires the recognition of a higher level in the realizational framework (Lamb 1972). Many cases of rule ordering can be shown to arise purely from properties of the mutational notation itself, rather than properties of linguistic structure; they have, therefore, nothing to do with stratification. As an example, consider these ordered mutation rules for converting from a (classical) phonemic representation to a broad phonetic one, from Chomsky (1964):

(1) a → a˙ in the context: __ (Glide) Voiced

(2) [t,d] → D in the context: Stressed Vowel __ Unstressed Voc.

The rules are illustrated as follows:

Phonemic:	dīsaydid (decided)	dīlaytid (delighted)
After (1)	dīsa˙ydid	dīlaytid
After (2)	dīsa˙yDid	dīlayDid

(Instead of the phonemic representations given above, Chomsky writes dīsayd#d and dīlayt#d, with # where I have written ɨ; and he denies that any of his representations has "any systematic character" that would justify the use of the term phonemic.) As Chomsky says of these two rules, "...clearly if [they] are not given in this order, the correct output will not be achieved," (V. Makkai 1972:405) since if (2) were to be applied to delighted first it would change the representation to a form which would allow (1) to operate.

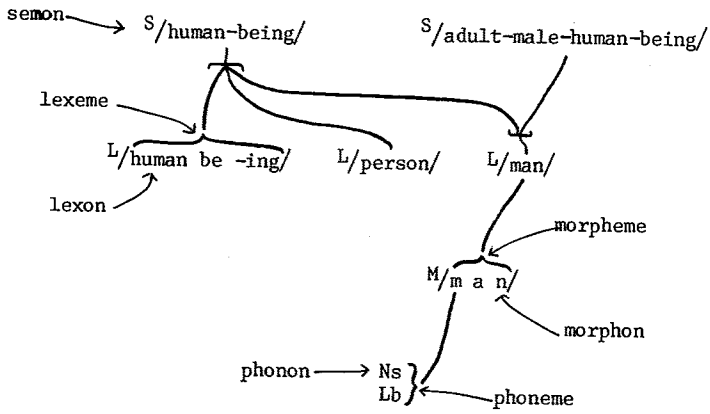
But if the rules are converted to realization rules, no such problem is present. The conditioning environment, for realization rules, is at the higher level; and no application of realization rules ever changes a representation on the higher level; it only specifies realizations on the next lower level. The corresponding pair of realization rules is thus unordered---the two are at one and the same realizational level.

How many strata are there, then? It is an empirical matter, and of course, in keeping with Occam's Razor, we want to recognize no more than are necessary to account for the data. What we find so far, at least for some languages, is that, far from 25 for phonology alone, only four strata are necessary for the entire linguistic structure. They may be called phonemic, morphemic, lexemic, and sememic. Between each pair of strata there is one layer of alternation of the type describable with realization rules and one layer of the kind accounted for by the next lower tactics. (Those who would like to use evidence about rule-ordering to show that more stratification than this is necessary must first translate their rules into a realizational format; they will find the exercise very instructive.)

Some properties of the tactic patterns are summarized in the following table:

Technical Term	Loose Characterization	Elementary Unit	Larger Units
Semotactics	Deep Grammar	Semon	Thought, Narrative, etc.
Lexotactics	Surface Grammar	Lexon	Word, Phrase, Clause, Sentence
Morphotactics	Deep Phonology	Morphon	Syllable, Phonological Word
Phonotactics	Surface Phonology	Phonon	Segment, Cluster, Prosody, etc.

Each stratum also has a collection of 'crystallized' or 'prefabricated' larger units composed of the elementary ones. These are the phonemes, morphemes, lexemes, and sememes. For example:



5. The last item I shall include in this list of consequences concerns performance. If we take the mutation out of mutational description, that is, if we remove the fictitious process, we find that it becomes possible to study the real processes that operate with the linguistic information. Ironically, it is because they are so wedded to the use of process description that the mutationists find themselves unable to describe the real processes. Their dilemma arises from an account of the linguistic structure which embodies processes that are clearly not those used by the speakers. How then are the real processes related to these fictitious ones? No answer has come forth, and none is likely to be forthcoming. The failure results not just from the presence of the fictitious processes but also from the distorted picture of the organization of linguistic structure that mutational description leads to---the structural levels obscured, the various tactic patterns hidden from view.

But with the linguistic structure accounted for as a stratified network of relations, we can apply processes of encoding and decoding in a realistic way. This is a large topic, and I shall mention here only one aspect of it. Since the encoding process is separate from the linguistic information, it can go back and forth among the various strata. It is this process, rather than the linguistic structure, which intermixes semantic, grammatical, and phonological information. For example, it is not the case that a semotactic representation of a sentence is formed first, then converted to a lexotactic representation, and so forth. Rather, the first part of a sentence can be realized all the way down at the phonemic level, can even be spoken, before the end of the sentence has been formulated at the semotactic level. And features of the realizations on the lower strata of the first part of the thought can influence the formation of the remaining part by the semotactics, since the speaker is decoding his own output as he encodes his thought.

In conclusion, it appears that just about all the important weaknesses of transformational grammar, in all of its various versions (including generative semantics)---and there are additional weaknesses that I haven't mentioned in this paper---can be traced to one basic defect: mutational description. Change just this feature of a transformational grammar---convert to a purely relational description of alternation---and then the 'rule-ordering' phenomena will sort themselves out to reveal a small number of strata, the systematic patterns of arrangement on the strata will be open to view, these tactic patterns will be seen to account automatically for some of the realizational phenomena. In short, the result will be a stratified network of relations. And it then becomes possible to deal realistically with encoding and decoding, not to mention language learning and linguistic change.

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The First LACUS Forum

In the Summer of 1974 the Linguistic Association of Canada and the United States (LACUS) held its inaugural meeting at Lake Forest College. The papers given at that meeting were edited by Adam and Valerie Makkai and were then photo-offset from IBM masters. They are now presented to the public in this volume as *The First LACUS Forum.*

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